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GRAPHIC METHOD ON DETERMINING AREAS OF RAILWAY CROSS-SECTIONS.

By M. DUBRET.

Translated from Annales des Ponts et Chaussées for VAN NOSTRAND'S MAGAZINE.

THE method of M. Villotte, of which the present article is an extension, was originally published in *Annales des Ponts et Chaussées*, in October, 1880. [See VAN NOSTRAND'S MAGAZINE for February, 1881. As M. Dubret's article seems to require the former one for complete elucidation, a portion of the original translation is herewith given.] He proposed to solve the problem of constructing the profile of a cross-section and estimating its area by the following simple method:

"It is clear that nothing more is necessary than to draw the profile of the original surface. This may be done in a ready manner by the aid of a prepared scale, graduated to the scale of trigonometrical tangents, as shown in Fig. 2.

"This little instrument is then applied to the axis of the cross-section diagram at the cut or fill height, and inclined at the angle of the natural surface.

"It only remains to follow with a pencil the edge of the protractor, in order to complete the half profile TNMP.

"The length of the slope and the breadth of base are read at once from the inclined scale drawn on the prepared card.

"It is required finally to determine the area TNMP. This is done in the following manner, which is only an application of a more general method for the measurement of areas.

"If we consider some point A arbitrarily chosen on the movable scale, but having a position mathematically defined, it follows that for each position of A, a corresponding point on the engraved sheet may be determined.

"We can, therefore, trace beforehand upon the prepared sheet a series of curves, giving the area of the half cross-sections for each position of the point A, and the problem is then solved by a simple reading.

"For reasons which will be immediately apparent, the point A is taken at A, the point of tangency of TN (original surface line), and the curve to which all the lines are tangent, which cut off areas equal to the area TNMP.

"To determine the nature of this curve we seek for the position of the point of tangency A, under the above conditions. This point will be at the intersection of TN and another line T'N', drawn infinitely near it in such way that the areas PMNT and PMN'T' are equivalent.

"The two infinitely small triangles TA_1T and NA_1N are equivalent. Hence the point A_1 is at the center of the line TN.*

* TN is throughout the original surface

Fig. 2.

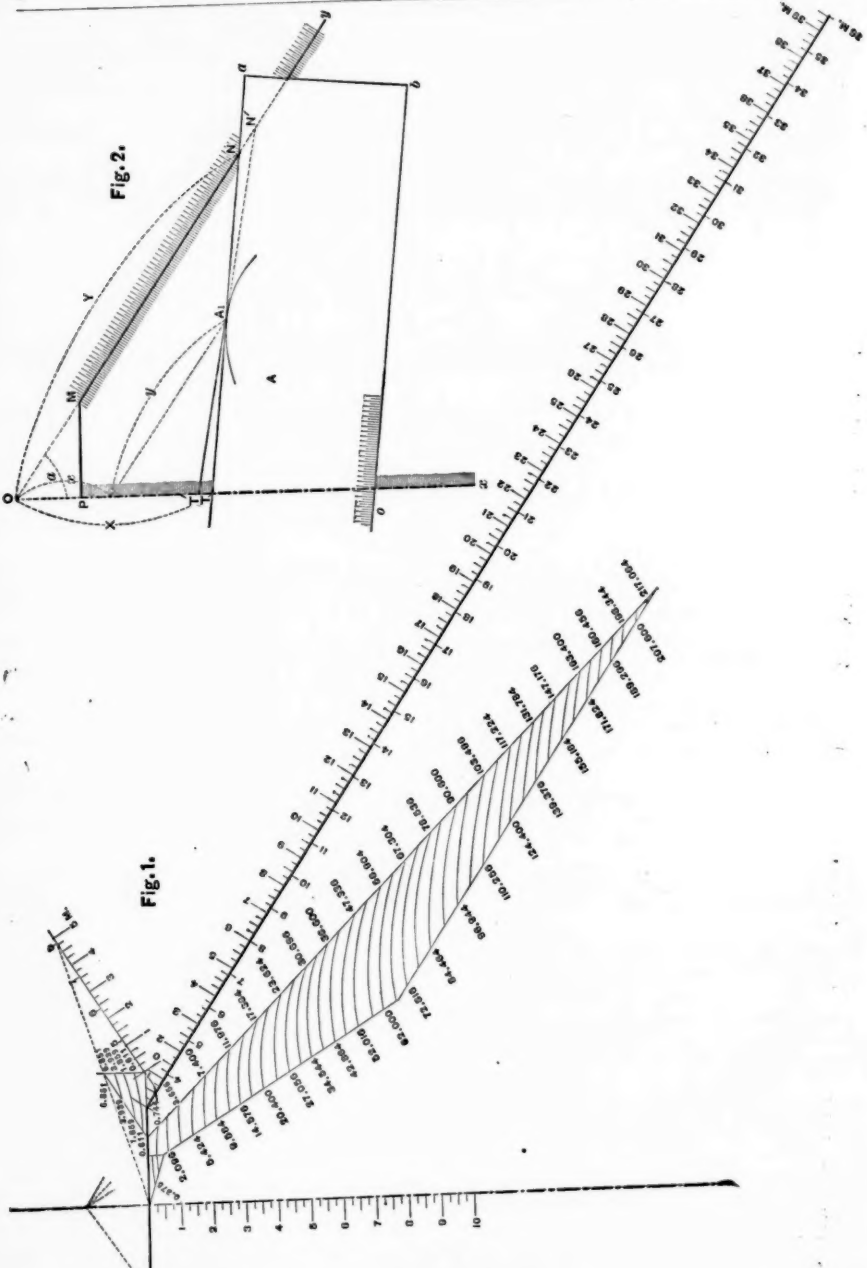


Fig. 1.

"To determine the equation of the curve of the locus of A_1 under the prescribed conditions. Let Ox and Oy be co-ordinate axes, and let $OT=X$ and $ON=Y$. The area PMNT being constant, the area ONT is also constant (so long as the line NT does not intersect the line PM within the angle xoy, a condition which is taken into account further on).

"Then we have

$$\text{area} \quad OTN=\frac{ON \cdot OT \cdot \sin\alpha}{2}=\frac{XY \sin \alpha}{2}$$

or $$XY=4K^2 \text{ a constant.}$$

"As x and y are the co-ordinates of A_1 we have

$$x=\frac{X}{2} \text{ and } y=\frac{Y}{2}$$

and $$xy=\frac{XY}{4}=K^2.$$

"The locus of A_1 is, therefore, a hyperbola having for asymptotes the axes Ox and Oy.

"For different values of K^2 we may obtain a series of hyperbolas corresponding to different values of the half section PMNT. These curves may be termed *curves of equal areas.*

"The advantages arising from the selection of the point A_1 under the assigned conditions, are obvious.

"1st. The curves of equal surfaces being hyperbolas drawn relative to the same point O, and to the same asymptotes, are easily traced. Any one is easily constructed by means of some one of its tangents, and others of the series are readily derived from this.

"The preparation of the table of areas is not at all complicated. An area is calculated corresponding to one of the curves. Other areas are determined by the law of the dimensions of similar surfaces, and the numbers may be written upon the curves.

"If the curves are traced in such a mrnner that they cut equal segments on one of the radii vectores from the point O, the *second differences* of these numbers are equal.

"2nd. For each of the different methods of choosing the point, there is a graphic table of determinate form; and for each table there exists a system of equal surface curves generated by the different positions of A corresponding to equal areas. The curves are generally of a degree above the second, but as we have already seen they become hyperbolas of easy construction when we take for auxiliaries points analogous to A_1.

"But the simplicity of construction is not the only advantage arising from this selection; for it is clear that it is an advantage to diminish as much as possible the length of the curves of equal surface; in taking A_1 the minimum of length is secured.

"Fig. 1 represents cross sections, comprised between the following limits:

"1st. Inclinations of surface varying from -0.33 to $+0.33$.

"2d. Depths or *fillings* at the center, of 10 meters.

"The prepared sheet would not be inconveniently large if both these limits were extended. A single sheet might serve for cross sections of different slopes.

"The mode of proceeding to employ the system is as follows:

"1st. Make a book of the cross sections, also a sufficient number of the proposed sheets to serve the purposes of the work in hand.

"2d. Write within the profile of each cross section, the center cut or fill and the slope of the original surface (expressed as a ratio or tangent of the angle made with the horison).

"3d. Proceed to trace the lines TN by aid of the movable scale (the inclination being measured by aid of graduations on the lower edge as at O in Fig. 2).

"4th. Read the numbers upon the curves to which in each case the line falls tangent, and write these areas within the respective cross sections.

"It may be remarked here that the tangency of the line TN is easy to determine, and moreover that an error of the first order, in determining the point of tangency, results in an error of the second order only in getting the areas.

"5th. Complete the calculations indicated for estimating the volumes.

"As the accuracy of the work depends so largely upon the accuracy of the prepared sheets, it will suggest itself that a shrinkage of the sheet would result in errors in the work. This does not necessarily follow except in cases obviously rare of unequal shrinkage in different directions.

"In place of the movable scale for the measurement of inclinations, a common straight-edge ruler may be employed and the inclinations be obtained, by using the method of drawing parallels from a set of prepared lines like Fig. 3, drawn to the different inclinations on each of the working sheets."

II.

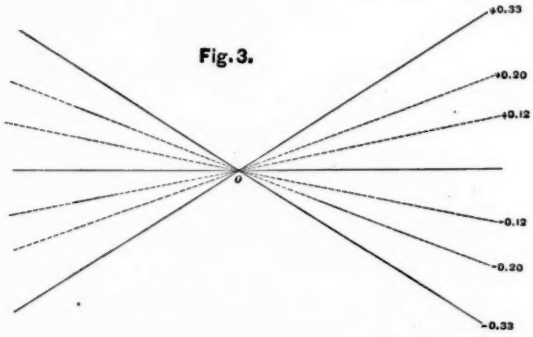
A simplification of the foregoing method is here suggested.

M. Villotte concludes the exposition of his method with some considerations relative to the shrinkage of the prepared sheets, which would tend to result in inaccuracies in the diagrams.

The instrument which is here proposed is designed to render the line of natural surface (the line T N of Villottes diagram) unnecessary. It is nothing more than a transparent protractor, in the form of a trapezoidal frame of wood or card-board, over the open part of which is stretched cloth or vellum, on which has been previously traced the different inclinations of the natural surface. It is necessary to fasten this vellum securely to the inner edge of the frame so that the horizontal line of the figure shall be accurately perpendicular to the basis of the trapezoid. Finally the graduations are made on a vertical line along the longer base.

This frame is made to slide against a ruler fixed to the left hand side of the table or board to which the diagram is firmly fixed, and is so adjusted that the graduated vertical shall constantly coincide with the axis of the diagram. When a line representing the natural surface of a cross section falls into the proper relation to the curves of the prepared diagram, it serves the purpose of the line TN of M. Villotte's system and the reading is at once made.

Fig. 3.



It is admitted that, notwithstanding such shrinkage, if the straight lines continue straight and the parallel lines parellel, the process would still be correct if the use of the protractor be abandoned and a sort of mariner's card be substituted on the corner of every sheet, having radial lines drawn in the direction of the different surface slopes. (See Fig. 3.)

It would be still better, however, to rely upon a single sheet, or two at most, one for excavations and the other for embankments. If drawn upon stout card-board they would not be liable to become faulty through shrinkage. They might be made upon a large scale so that the number of hyperbolas might be increased, and a greater accuracy of results obtained.

It is true no trace of successive problems would be preserved, but it would always be easy to verify a doubtful result, and thus the principal object of the work would be obtained.

Without making the instrument of inconvenient size, all the inclinations from $+0.30$ to -0.30 from millimeter to millimeter, can be drawn. But experience shows that it is not necessary to draw them all. Alternate lines may be omitted without impairing the usefulness of the instrument.

Fig. 2 (plate) is a partial reproduction of a diagram, representing a road bed of $4.^{m}55$ width. The divisions along the axis are in centimeters. The curves correspond-

ing to the heights are multiples of 5 centimeters; and the inclinations are between +0.50 and −0.50. The spacing is sufficient to permit the eye to follow the line to the extremity, but to avoid confusion the alternate lines are made heavier. On one side are numbers showing the areas, on the other are numbers written in red ink forming a kind of interpolation table, as each of them is $\frac{1}{5}$ of the difference of the two numbers against which it is placed. This facilitates the estimate of the value of a space when the surface line falls between two curves without being quite tangent to either. In such a case, if the position of the line be estimated by the eye to the nearest fifth of the space, then the *difference* referred to multiplied at most by 2 gives the correction to be applied to the nearest tabular number.

The diagram is drawn to working size upon a sheet of bristol-board $0^m.53 \times 0^m.48$. It does not admit of numbers along the axes higher than six meters, for with a road-bed of $4^m.55$ width, it is not indispensable to provide curves corresponding to greater heights, as by means of the following formulas deduced by M. Villotte in his essay, they suffice to supplement the diagrams so that excavations not exceeding 15 meters or embankments of 13.5 meters are provided for.

The following are the formulas:

If we let S_c represent the area of a half section of which the number on the axis is c; by l the half width of the road-bed and by t the slope, we have by taking AB $= \frac{AC}{2} = \frac{lt+c}{2}$ and supposing BD parallel to CF (Fig. 3, plate).

$$OB = \frac{c-lt}{2}$$

and

$$\frac{\frac{S_{c+lt}}{2}}{\frac{S_{c-lt}}{2}+\frac{l^2t}{2}} = \frac{(c+lt)^2}{(c+lt)^2} = 4.$$

From this we get:

$$4S_{\frac{c-lt}{2}+\frac{3l^2t}{2}}$$

which gives, in case of a road-bed $4^m.55$ wide, not including a ditch of $0^m.75$, for the embankment,

$$S_c = 4S_{\frac{c-lt}{2}+5^m.18},$$

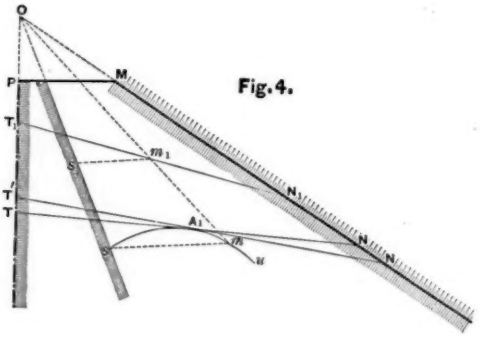


Fig. 4.

for the excavation,

$$S_c = 4S_{\frac{c-lt}{2}+13^m.35}.$$

We find also for the length of the slope of the embankment,

$$T_c = 2T_{\frac{c-lt}{2}+2.73}.$$

of the cutting,

$$T_c = 2T_{\frac{c-lt}{2}+3.03}.$$

By the aid of the transparent protractor above described, the areas of fifty cross sections, with the length of slopes can be determined per hour. It is necessary to say that previous to this operation, the inclination of the natural surface for each cross section together with the cuttings and fillings have been properly recorded.

Furnished with these data an assistant determines upon the diagram the un-

known elements of each half-section and reads them aloud. Another writes them upon a sheet prepared in advance and adds them up.

The process, as is easily seen, is very expeditious. It is also in a high degree accurate; for the maximum error in estimating a surface is at most one-half of the amount expressed by one of the small difference numbers. That is to say, that for a diagram constructed to a scale of $\frac{1}{200}$, the area of a half profile measuring forty square meters, the error would at most not exceed $0^m.05$. and generally would be less than $0^m.04$.

III.

These diagrams of M. Villotte are susceptible of an easy generalization, aside from the question of mixed profiles.

It would suffice to prolong the homothetic hyperbolas to their center of similitude, to take this point as the origin of axial graduations, and for each change of width, to determine the value of the constant which should be added to each of the numbers along the axis before proceeding to the operations.

The problem of mixed profiles is evidently not easily solved in the same manner, that is to say, by the construction of hyperbolas to the two angles of the road-bed.

The means to be suggested do not constitute a generalization in the strict sense of the term, but they require only the drawing of one or two right lines and the establishing of a complementary graduation when we wish to pass from one section to the other, and even in ordinary cross sections, the method presents, as a generalization some advantages.

Let ABCD, Fig. 4 (plate) be a pattern diagram drawn for a road-bed whose width is L. And let TN be the line of natural surface of a half section of a width $L+l$, The area of this half section is to be obtained by adding to the surface given by the hyperbola tangent to TN, the area of the trapezoid CFNN'. Now let a horizontal line IK be drawn through the middle O of NN'. The two triangles ION' and NOK are equal, and consequently the parallelogram CFIK is equivalent to the trapezoid CFNN'.

Now the locus of the point O being the parallel RS of the slopes of the two road-bed linesthrough the middle R of CF; if we divide the space between the slopes into sections of equal area by equidistant horizontal lines, so that the intersections of these horizontals and the line RS will establish a graduation analogous to that of the curves of the original diagram, we shall have constructed upon this line a scale of surfaces which will give the area of the trapezoid to be added to such half section.

The line FG although of no use in the determination of the area should be traced upon the diagram because it is that which determines the length of the slopes and the width the work. The scales constructed upon CD can be dispensed with, for it will be sufficient to follow the horizontal passing by N in order to read upon the scales the length FN=CH of the slope and of the width PH which is equal to the whole width diminished by the constant l.

The scale of surfaces mentioned above corresponds evidently with those of the half-sections which are either all cutting or all filling. They are of no use when the line TN intersects the road-bed between C and F (Fig. 5), and gives rise to a mixed cross section.

In such a case we can obtain the area of the excavation by a simple displacement of the protractor.

For the surface of the embankment; if we observe that the two triangles CNM and CPM, which have the same base and same altitude are equivalent, the method of determining this area is already indicated. It consists in placing the protractor in such a manner that one of the lines of slope intercepts upon the sides of the right angle formed by the axis of the diagram, and the road-bed two distances respectively equal to the height and base of the triangle CNM. It is understood, of course, that the first reading has given the area of the quadrilateral BCNT, the lengths CM, MF and the height CP, which the previous construction rendered necessary; the dimension of the road-bed; the new rules having for their origin the points B, C, F, and disposed as shown in Fig. 5.

Thus a scale of surfaces for an entire cross section, either of excavation or embankment—a scale of lengths for the mixed half cross sections, in which

the point of division falls at a distance from the axis greater than the half width of the primitive road-bed—such are the additions to be made to the tables for each change of diagram, upon the hypothesis that diagrams are to be prepared for the smallest details.

These additions only require a few minutes. The scales BC, CF and CB, and the equidistant horizontal lines, can be traced in advance once for all.

The new profiles are calculated to be narrower than the former by the reduction of the width of the road-bed line on the side of the axis (Fig. 6).

The complementary scale of the surfaces becomes vertical and divides the space between the parallels AB and A′B′ into two bands of equal width. There is no further need of metric scales along the road-bed, but a new scale of red line ought to be made upon the vertical A′B′ which represents the axis of the rectified diagram.

We remark, further, that in the first hypothesis we could much more easily enlarge the diagram on the side of the axis than on that of the slope, which would render equally unnecessary the scales on the road-bed; only it would be necessary then to reproduce above the latter a part of the hyperbolas prepared for the excavations. Figure 7 shows the new arrangement, also the proposed displacement of the protractor when in use on mixed cross sections.

The advantages which this method of generalization presents may be enumersted as follows:

1. Corrections of the figures along the axis are avoided.

2. There is no necessity for so large a number of hyperbolas.

3. Finally the first system affords no practical means of determining the area of cross sections—part excavation and part embankment. The latter system reduces the problem to a simple displacement of the protractor which is in use throughout.

WATER GAS.*

The important results which in the past decade have followed the introduction of generator gas [a] (see Appendix) as a heating agent, into many branches of industry, suggests the expectation that gaseous fuel may find more general application, and may be used on a small scale as well as in the larger operations which have heretofore received its benefits. For household uses a gaseous fuel, supplied through pipe, offers many advantages when we consider the difficulty of managing a fire using coal or wood, the labor of starting the fire, and especially when we remember that scarcely more than 10 per cent. of the total heating power of fuel is utilized as it is commonly burned, while more than 80 per cent of the heat of combustion is believed to be available when gaseous fuel is employed. A cheap heating gas would be of especial service to the smaller trades, because gas motors may be made to serve the ends of those trades demanding only small power, while the outlay for a steam engine presupposes the need of heavier work. That the advantages of heating by gas will be more and more appreciated in daily life is to be expected, when we consider the increasing application of illuminating gas to heating purposes, such as the warming of rooms, cooking, and especially for actuating gas motors for small applications of power.

A gas intended only for heating may be prepared at a cost considerably less than that of illuminating gas, since it may be made from the most inferior combustible material, while the choice of material for the manufacturing of illuminating gas is quite limited. The proposal of Siemans, made twenty years ago, to supply cities with heating as well as lighting gas through pipes, remains as yet unfulfilled[b] only because such pipes require a heavy outlay, since it is well known that distribution involves the heaviest expense of gas-making plant. Since the manufacturing of lighting gas, as such, is a necessity, while it serves very well also the purpose of a heating agent, and since, moreover, the importance of heating by gas has but recently been made apparent, it is questionable whether the manufac-

* A paper by Prof. C. v. Marx, read before the Engineering Society of Stuttgart. *From Repertorium der Analytischen Chemie, Nos.* 13, 14, 1882. Translated by Prof. A. A. Breneman, with an appendix by the translator.

ture of such gas would have been profitable heretofore. With the development of electric lighting, however, the *status* of heating gas must sooner or later be changed.

It is now known that more light is obtained when a given volume of illuminating gas is used in a gas engine which in turn acts through an electro-motor to produce the electric light, than when the same volume of gas is burned directly from a gas burner. In view of this fact it cannot be concealed that illuminating gas must soon lose its importance as a lighting agent, and the suggestion follows that illuminating gas works will in time be converted into manufactories of heating gas.

The relation between electric light and gas light is shown in the following table by Alfred Niaudet, in which the light, obtained by different observers using different forms of lamps, is expressed in carcels, the power exerted in each case being 75 kilogrammeters per second=1 horse power, nearly.

		Carcels.
1. Arc Light.	Carbons 10 c. m. apart, a maximum distance which cannot well be exceeded in practice. Common Gramme machine. (Fontaine.)	285
2. " "	Carbons 3 c. m. apart, common distance, Gramme machine. (Fontaine.)	230
3. " "	Number given by the President of the Committee on Lighting by Electricity; 2,400 candles; 9-6 candles = 1 carcel.	250
4. Jablochkoff candle.	Gramme machine with alternating current. One candle uses $\frac{5}{6}$ H. P. (Honoré), and gives a light of 41 carcels (Jouber), *i. e.* per H. P.	49.2
5. Edison's Incandescent lamp.	Number given by Rowland & Barker.	16-21
" "	Number given by Bracket & Young.	19
6. Swan's Incandescent lamp.	150 candles, 9=1 carcel.	16.66

The average consumption of illuminating gas in a gas motor is one cubic meter per H.P. per hour.(c) On the other hand, an Argand burner using 150 liters per hour gives a light of 18 candles or 120 candles per cbm. At 9 candles per carcel only, this is 13.33 carcels, which is below most of the figures given above, notably so for arc lights. Incandescent lamps, however, are already in competition with gas. In the combustion of illuminating gas a large part, comparatively, of the chemical energy takes the form of heat, and less appears as light; but in the production of the electric light, the proportion of light to energy expended is much greater.

Since the electric light for the illumination of large areas with light of a given intensity is cheaper than gas light, it may be expected that the attention paid to the problem of electric lighting at present may result in making it also cheaper for lighting on a small scale. Illuminating gas will then have been crowded from the field, while heating gas will have a new claim to consideration.

If it be asked in what way a cheap heating gas is to be prepared, it is evident that it must be by a process different, at least, from that used in making illuminating gas, since by the process of dry distillation as now employed, only a small portion of the combustible matter is converted into gas. Gas-coal yields in this way about 28 cbm. of gas, weighing 14.4 kilos. for each 100 kilos. of coal, leaving a residue of 66 kilos. of coke, of which fully 20 kilos. are used again to heat the retorts, and more than 40 remain unused in the process. A method which could entirely convert the carbon of coal into gas would be far preferable. It has been proposed, however, to obtain heating gas by dry distillation of the brown coal of the Fürstenwalde, 35 kilos. from Berlin, and to carry the same above ground to that city in strong sheet-iron pipes, where it would be stored in twelve large holders and distributed from them in pipes to the consumers. A solid fuel may be completely converted into gas by imperfect combustion, as occurs in the generators of large furnaces where the carbon of the fuel is changed to carbonic oxide by oxygen from the air, and this combustible gas is then burned to carbonic acid in the heating chamber of the furnace. Carbon, in burning to carbonic acid,

yields 8080 heat units in the aggregate, and of these 2473 are developed in the production of carbonic oxide, the first stage of the process. When commercial gas is made, *i. e.*, gas which is to be conveyed to the consumer in pipes, this latter component, 30.6 per cent. of the total heat, is lost by the cooling of the gas. Generator gas contains 70 per cent.[d] of nitrogen also, on an average, which takes no part in the combustion. Assuming the air to contain, in round numbers, 20 per cent. of oxygen and 80 per cent. of nitrogen, and remembering that one volume of oxygen in combustion yields two volumes of carbonic oxide, 100 volumes of air should yield a mixture of 80 of nitrogen and 40 of carbonic oxide, or $66\frac{2}{3}$ per cent. of nitrogen. This gas, of which two-thirds is worthless for heating purposes, cannot profitably be distributed through mains, and generator gas must therefore be impracticable as a commercial gas.

The preparation of water gas, however, presents a much more favorable aspect. This depends upon a well-known reaction, by which glowing coal is made to decompose water led through it in the form of steam. Any form of carbon will answer the purpose, wood-charcoal and coke yielding equally good results. Carbon appropriates the oxygen of water, and hydrogen is set free. The production of a combustible gas by the action of glowing coal on steam was first carefully investigated in 1801, but the fact of its formation was known much earlier. This decomposition of water by coal may take place in either of two different reactions, carbonic oxide resulting from one reaction, carbonic acid from the other, and hydrogen being set free in either case.

$$\begin{aligned} &C + H_2O = H_2 + CO \\ (1)\quad &12 + 18 \;\; = \;\; 2 + 28 \\ &2 \text{ vols.} = 2 \text{ vols.} + 2 \text{ vols.} \end{aligned}$$

$$\begin{aligned} &C + 2H_2O = CO_2 + 2H_2{}^{(e)} \\ (2)\quad &12 + 36 \;\; = \;\; 44 + 4 \\ &4 \text{ vols.} = 2 \text{ vols.} + 4 \text{ vols.} \end{aligned}$$

The first process is effected with excess of steam and at a high temperature; when the temperature falls carbonic acid is produced. The decomposition begins at about 600° C. In practice both processes go on together, and although it is impossible to produce water gas free from carbonic acid yet in the practical working of the water gas process it must be so directed as to approximate as near as may be to the form of Equation I. The following data are useful for calorimetric calculations:

TABLE I.

1 kilogramme	Products of combustion in kilogrammes.	Heat of combustion in kilo. heat-units.
Hydrogen.....	9.00 H_2O	34180 (Thomsen)
Carbon.....	2.33 CO	2473
"	3.66 CO_2	8080
Carbonic Oxide	1.57 "	2403
Marsh Gas (CH_4)	2.75 "	13346 (Thomsen)
" " "	2.25 H_2O	
Ethylene (C_2H_4)	3.14 CO_2	11960 "
" "	1.30 H_2O	

TABLE II —(Calculated from Table I.)

1 cubic meter.	Weight in kilogrammes at 0° and 760 m.m.	Heat of combustion of 1 cubic meter in kilo heat-units.
Hydrogen.....	0.0896	3063
Carbonic Oxide	1.2544	3014
Marsh Gas	0.7158	9566
Ethylene.....	1.2544	15099

Taking the gases at 0° and 760 m.m., for convenience, it results from the above that equation I yields a water gas composed of equal volumes of carbonic oxide and hydrogen with a combustion heat of $\frac{1}{2}(3063) + \frac{1}{2}(3014) = 3038$ units.[f]

Water gas obtained by equation II contains $\frac{2}{3}$ volume of hydrogen and $\frac{1}{3}$ volume of carbonic acid with a combustion heat of $\frac{2}{3}(3063) = 2042$ units.

If purified by absorbtion of carbonic acid from the mixture, a process involving some expense, this gas has still a lower calorific power per cubic meter than the first, being simply hydrogen with a combustion heat of 3063 units. Equation I is therefore preferable in practice as a guide to the process. In order to decompose water a definite expenditure of heat is required, the amount bound up in the decomposition being simply that which would be evolved again when water is reproduced by combustion of hydrogen. When we decompose water by hot carbon, oxygen combines with the latter and heat is set free. The difference of these quantities is the quantity of heat taken from external sources in the man-

ufacture of water gas. According to Equation I:

$$2 \times 34180 = 68360$$
$$-(12 \times 2473) = 29676$$
$$38684 \text{ units.}$$

1 kilo. of carbon requires $\frac{38684}{12} = 3224$ units and to develop this quantity of heat 0.40 kilo. of carbon must be completely burned, *i. e.*, $\frac{3224}{8080} = .40$.

Taking now Equation II, we have

$$4 \times 34180 = 136720$$
$$-12 \times 8080 = 96960$$
$$39760$$

and $\frac{1}{12}(39760) = 3313$.

To develop this heat requires the complete combustion of $\frac{3313}{8080} = 0.41$ kilo. of carbon, rather more than for Equation I, but the gas produced according to Equation II, has a heating power somewhat higher in the aggregate.

Composition of Water Gas.

The theoretical composition of water gas, as already shown, is 50 per cent. nitrogen and 50 per cent. carbonic oxide; in practice, however, the product always contains more or less carbonic acid, since it is impossible to prevent entirely the decomposition of water according to Equation II, and the constituents of the air, oxygen and nitrogen, are also present in small quantity as would be expected from the nature of the apparatus. When uncoked coal is used the gas is also mixed with hydro-carbon gases resulting from dry distillation. The following table shows the composition of water gas from different sources, and, for comparison also, the composition of generator gas and illuminating gas, both of the latter being averages of many different analyses.

Table III.

Water Gas.	CO_2.	CO.	H.	CH_4.	C_2H_4.	N.	O.
Strong's Apparatus. (Dr. Moore)	2.1	35.9	52.8	4.1	..	4.4	0.8
Anthracite, American. (Quaglio & Dwight)	2.1	35.4	52.8	4.1	..	4.4	0.8
English Coal. Stockholm	4.0	40.0	49.0	6.0	..	1.0	
Coal from Hägenäs	2.6	34.8	59.6		..	3.0	
Anthracite from Wales	3.6	34.1	61.3		..	1.0	
1 part coke and 3 parts dry peat	7.0	35.5	57.0		..	0.5	
1 " " 3 " wet "	9.0	33.4	57.1		..	0.5	
1 " " 3 " Eng'l coal dust	6.8	35.0	57.2		..	1.0	
Coke in Frankfort. (Dr. Bunte)	7.3	34.5	50.0		..	7.5	0.7
Illuminating gas	1.00	6.0	47.0	38.0	5.0	2.5	0.5
Generator gas	6.00	23.0	1.0	..	..	70.0	..

Yield of Gas.

Taking the theoretical composition of water gas and applying the data of Table II, we have for the weight of one cubic meter at 0°,

$$\frac{1}{2} \times 1.2544 = 0.6272$$
$$\frac{1}{2} \times 0.0896 = .0448$$
$$.6720 \text{ kilo.}$$

and according to Equation I,

12 kilos. C yield 30 kilos. of water gas.
∴ 1 " " " 2.5 " " "

But 2.5 kilos. of gas $= \frac{2.5}{.672} = 3.72$ cubic metres. As already shown 0.4 kilo. of carbon must be burned to convert 1 kilo. into gas by supplying the necessary heat. To obtain 3.72 cbm. of gas, therefore, we must use 1.4 kilos. of carbon, or 1 kilo. of carbon yields theoretically $\frac{3.72}{1.4} = 2.65$ cbm. of water gas. In practice, however, the yield is much lower. In Stockholm, for example, 1 kilo. of coal yielded 1.416 cbm. of water gas, and in Frankfort, 1.225 cbm., or, admitting with Dr. Bunte that coke leaves a residue of 20 per cent. of of ash and dust, we obtain from 1 kilo

of carbon $\frac{1.225 \times 100}{80} = 1.531$ cbm. of gas, *i.e.*, 58 per cant. of the theoretical yield. This small yield is to be ascribed especially to the great waste of heat which the process involves, the consumption of heating coal being much greater than theory requires.

Loss of Heat in the Preperation of Water-Gas.

Taking as a basis the figures of Dr. Bunte's experiments at Frankfort,(f) 1 kilo. of coke, containing 80 per cent. of available carbon, yields by direct combustion $0.8 \times 8080 = 6464$ heat units. 1 kilo. coke yields 1.225 cbm. of water-gas, which, using the data already given, develops in burning $0.345 \times 3014 + 50(g) \times 3063 = 2571$ units per cbm. ∴ 1,225 cbm. yields $1.225 \times 2571 = 3149$ units. But 1 kilo. of coke yields 6464 units, the water gas prepared from it therefore yields in combustion only 48.7 per cent. of the original heating power of the coke.

The greatest loss of heat according to this investigation results from the escape of the hot gases into the chimney during the heating period, *i. e.*, while the blast of air is urging the fire to its greatest heat preparatory to the admission of steam. The chimney gases escape at a temperature of 660°C. and the loss from this source is estimated by Bunte at 23 per cent. The finished gas leaves the furnace at 500°C. causing a loss of 3 per cent. and the radiation of heat from the walls of the furnace, which have an area of 40 sq. meters and an average temperature of 140°C., involves a loss of 11 per cent. The remaining difference of 14 per cent. must be ascribed to other imperfections incident to the process. The expenditure of heat in making steam for the furnace and in driving the fan is not taken into account in the calculation.

Comparison of the Heating Power of Water Gas, Coal Gas and Generator Gas.

This comparison may be most simply made by calculating the combustion heats developed by 1 cbm. of each gas under consideration, or more definitely, the number of kilogrammes of water that may be heated 1.°C by the combustion of 1 cbm of the gas. Take, as an example for such calculation, coal gas with the composition given in Table III.; 1 cbm. of such gas contains, of combustible gases,

CO. 0.06 cbm.	CH_4. 0.38 cbm.
H. 0.47 "	C_2H_4. 0.05 "

Using the data of Table II. we have for the combustion heat of the gas, $.06 \times 3014 + 0.47 \times 3063 + 0.38 \times 9566 + 0.05 \times 15099 = 6010$ units.

The combustion heats given in Table V. under the head of available heat in combustion products, "0.°C" are calculated in the same way.

It must be observed, however, that in these operations the products of combustion are supposed to have been cooled to 0.°C and that all vapor of water has become liquid. In practice this assumption is not realized, since the products of our fires escape at a temperature of at least 200°C. The relative combustion heats of the different gases are altered when we calculate the useful effect of a heating gas of which the products escape at 200°C. The heating power of gases containing hydrogen is lowered under these conditions by reason of the high latent heat of steam and its relatively great specific heat. In the calculation also it is simplest to deal with volume relations only and to calculate the specific heat of a standard *volume* of the gas, giving an expression, for example, of the number of heat units required to effect a change of 1°C in a cubic meter. When the specific and latent heat of water have to be introduced into the calculation it is simplest to regard water as a permanent gas which requires 637 units to bring it from 0° to 100°C and above 100° has the specific heat 0.475. Applying these principles we have the following table:

Table IV.

	Latent and specific heat.	1 cbm. gas at 0° and 760 mm. weight kilos.	Latent and specific heat of 1 cbm. gas.
Water.....	637.	0.8064	514.
	0.475		0.383
Carbonic acid.....	0 2164	1.9712	0.4266
Nitrogen.....	0 244	1.2544	0.3061

The numbers of the last column are

products of the preceding columns of figures.

The combustible constituents of the kinds of gas already considered are CO, H, CH_4 and C_2H_4, and the combustion reactions of these, using molecular formulæ, are:

(*a*) $2\,CO + O_2 = 2\,CO_2$
4 vols. + 2 vols. = 4 vols.

(*b*) $2\,H_2 + O_2 = 2\,H_2O$
4 vols. + 3 vols. = 4 vols.

(*c*) $CH_4 + 2O_2 = CO_2 + 2\,H_2O$
2 vols. + 4 vols. = 2 vols. + 4 vols.

(*d*) $C_2H_4 + 3\,O_2 = 2\,CO_2 + 2\,H_2O$
2 vols. + 6 vols. = 4 vols. + 4 vols.

In these equations the water is considered as gaseous, which is only the case when the temperature is above its own boiling point.

If we consider that the combustion products escape at 200°C, combustion in air requires not only that these be heated to 200°, but also that the nitrogen, which accompanies oxygen in the air, be raised to the same temperature. Taking the composition of air at 20 per cent. O and 80 per cent. N, instead of 20.9 per cent. and 79.1 per cent., the exact proportions, we can calculate from the data of Tables II. and V. the heat taken off by the whole volume of gases escaping from the furnace at 200°C, and hence the residue of heat available for work.

Since 1 cbm. of carbonic oxide in burning yields an equal volume of carbonic acid, and takes up in the process ½ cbm. of oxygen and $4\times\frac{1}{2}$ cbm. of nitrogen (equation *a*), the available heat for 1 cbm. of CO will be $3014-(200\times0.4266)-(200\times0.3061)\times4\times\frac{1}{2}=2806$ units. For hydrogen (Table II. and equation *b*), $3063-514-(100\times0.383)-(200\times4\times\frac{1}{2}\times0.361)=2388$ units. For marsh gas (Table II. and equation *c*), $9566-(200\times0.4266)-(2\times514)-(2\times100\times0.383)-(200\times0.3061\times4\times2)=7886$ units. For ethylene (Table II. and equation *d*), $15099-(2\times200\times0.4266)-(2\times514)-2\times100\times0.383)-(4\times3\times200\times0.3061)=13089$ units.

With the aid of the numbers thus obtained, the calculation of the available heat, supposing the gases to escape from the furnace at 200°C, is very simple. As an example, we may take illuminating gas with the composition given in Table III.: $.06\times2806+(0.47\times2388)+(0.38\times7886)+(0.05\times13089)-(0.01\times200\times0.4266)-(0.025\times200\times0.3001)=4942$ units. Results obtained in the same way for the gases of Table III., are given in the second column of figures in the following table:

TABLE V.

Water Gas.	Available heat in combustion products.		Theoretical flame temperature.
	0°C.	200°C.	°C.
Strong's Apparatus	3090	2590	2620
Anthracite, American	3080	2570	2610
English coal, Stockholm	3280	2760	2620
Coal from Hägenäs	2870	2400	2660
Anthracite from Wales	2910	2420	2660
1 part coke and 3 parts dry peat	2820	2350	2630
1 part coke and 3 parts wet peat	2760	2290	2600
1 part coke and 3 parts Eng. coal-dust	2810	2340	2630
Coke, Frankfort	2570	2150	2560
Illuminating Gas	6010	4940	2460
Generator Gas.	724	621	1470

In the third column of the above table are the theoretical flame-temperatures, *i. e.*, the temperatures that would be attained by the flame of burning gas when supplied with exactly the required quantity of air and supposing the entire quantity of heat developed to be applied to raising the temperature of the products of combustion and the accompanying nitrogen. As an example of the method by which this temperature is calculated, take the gas made in Strong's apparatus, Table III, containing in 1 cbm. 0.205 CO_2, 0.359 CO, 0.528 H, 0.041 CH_4 and 0.044 N. From Tables IV and V and equations (*a*), (*b*) and (*c*) we have,

$$3090 = 0.4266\,(0.205+0.357+0.041)\,t + 514\,(0.528+2\times.041) + 0.383\,(0.528+2\times0.041)\,(t-100) + 0.3061\,(0.044+4\,[\tfrac{1}{2}\times0.359+\tfrac{1}{2}\times0.528+2+0.041])\,t.$$

Whence $t=2620$°C.

Inspection of Table V indicate that water gas has about four times the

heating power of generator gas and that common illuminating gas has more than double the heating power of water gas. The flame temperatures attained by the different gases agree more nearly with one another.

To What Extent is the Water Gas Process Worthy of Practical Application?

Theoretically nothing is gained or lost in the water-gas process; the same quantity of air is necessary to burn the gas as to burn the coal from which it was originally made.

$$C + O_2 = CO_2$$
$$C + H_2O = H_2 + CO$$
$$H_2 + CO + O_2 = H_2O + CO_2$$

The process serves only to transform solid fuel into gas, in which latter form it can commonly be burned with better effect. On the whole there is a loss of heat however, since the water which serves the ends of the process must finally be heated to the temperature at which the products of combuotion escape and its relatively high specific and latent heat must be taken into account. It follows from this that for large heating operations a special generator, close to the heating chamber, is better than a water gas furnace, and that even the simultaneous introduction of steam and air into the generator is disadvantageous, although in this case none of the heat produced in the generator is lost, because the immediate transfer of the hot gas into the heating chamber leaves no opportunity for the heat to escape.(h) The case is different when the gas developed in the generator is allowed to cool before reaching the heating chamber; in this case it is worth while to use the heat of the generator in part for the production of water gas the greater part of the heat taken up in the decomposition of water in the generator being set free again in the heating chamber of the furnace, *i. e.*, it is useful in this case to pass a mixture of steam and air through the generator and thus to produce a mixture of water gas and generator gas.

For the purpose of a commercial gas, water gas presents decided advantages over generator gas when we consider the great cost of mains for distribution of the gas. The greater the heating effect of the gas the greater is the practicability of distributing it and the more effective is the system of pipes by which it is carried. As the heating power of water gas is about four times that of generator gas and as the distribution of the latter requires the conveyance of 70 per cent. of worthless nitrogen through a costly system of pipes, it may be dismissed from consideration as a commercial gas. Illuminating gas, however, has a certain advantage over water gas as this has over generator gas, since its heating power is about double and the cost of producing water gas must be considerably lower than that of illuminating gas in order that it may compete with it as a heating material. The possibility of this, flows, however, from the fact that the cheapest combustible matter may be used and that the whole material is converted into gas by a process which is much cheaper in efficiency and labor than the ordinary coal-gas process. It may perhaps even now be of advantage in large gas works to use a water-gas furnace in connection with the retort-furnace during the time of greatest consumption in winter, and to prepare carbureted water gas to mix with the ordinary gas. Such gas is easily made of illuminating power equal to that of the best coal gas.(i).

The superior efficiency of the water-gas furnace is apparent when we consider that an ordinary retort yields only 150 cbm. of coal-gas in 24 hours. In Frankfort 2500 cbm. were made daily, corresponding to the production of 17 retorts. Quaglio asserts that such furnaces may be made to do the work of 60 retorts and will be cheaper in cost of working as the production becomes greater.

If water gas can be made sufficiently cheap, which can hardly be questioned, and if gas lighting should be surpassed in the future by electric lighting, illuminating gas will not even then be entirely abandoned and heating gas will be carbureted by the consumer at his house by a special apparatus, using petroleum naphtha or some such material to give luminosity to the gas.

Water gas, owing to its high per cent. of carbonic oxide is very poisonous, as also is common illuminating gas. The latter has, however, the estimable property of smelling so strongly that $\frac{1}{10000}$ of it can

be detected in the air by its odor. Water gas, on the other hand, has scarcely any smell.(g) This last objection may be met by mixing with the gas some strongly-smelling volatile matter, a device which prevents no great technical difficulty. It appears from the preceding that the water gas process, under certain conditions, has a future before it, and in a limited sense it may be justly designated as the "fuel of the future."

APPENDIX.

(*a*) The term "generator gas" wherever it is used in this paper, refers to the gas obtained when air is passed through a thick bed of incandescent coal, as in the generator of the Siemens furnace.

(*b*) Such a system is now in operation in Yonkers, N. Y. Fuel gas, made by the Strong process is supplied to consumers through street mains.

(*c*) This refers to net H.P. The consumption per I.H.P. is about 22 cu. ft. ; 1 cbm.=35 cu. ft., approximately. In regard to the use of water gas in gas engines, see a paper by Prof. W. E. Ayrton, "On the Economical Use of Gas Engines for the Production of Electricity." (London : Wm. Dawson & Son.) This paper relates to experiments made with the "Dowson" gas, which is practically a mixture of water gas and generator gas. Also an article in *Sci. Am.* Suppl. No. 305 on the Dowson process. From some preliminary experiments made by the translator using the strong water gas in an Otto engine, he feels warranted in stating that the consumption of this gas per H. P. (net) in such an engine lies between 60 and 70 cubic feet per hour.

(*d*) Here, as in all cases in this paper, percentage compostion is indicated in *volumes*.

(*e*) Here, and in similar equations, the volume of the hydrogen *atom* is regarded as unity, and the figures of the author, who regards the molecule (2 vols.) as the unit of volume, are doubled. The *volume ratios* remain, of course, unchanged.

(*f*) Quantities of heat in this paper are expressed in calories or kilogramme units, *i. e.*, the quantity of heat required to raise the temperature of one kilo. of water from 0°C to 1°C.

(*f'*) See "Wagner's Jahresbericht," 1881, p. 1049.

(*g*) Assuming for simplicity that this 50 per cent. is all hydrogen.

(*h*) In practice with the Siemens furnace the generator gas is cooled by passing through an iron pipe exposed to the air.

(*i*) The extensive use of the Lowe, Motay and other processes for manufacture of carbureted water gas in America adds emphasis to this statement.

(*j*) A small quantity of sulphur, such as water gas commonly contains, and which it is, for many purposes, unnecessary to remove, suffices to give a decided odor to the gas. This sulphur exists largely if not entirely as sulphureted hydrogen, and is easily removed by absorbents when its removal is demanded.

THE MICROSCOPICAL STRUCTURE OF IRON AND STEEL.*

By DR. H. C. SORBY, F. R. S.

From "Iron."

THE lecturer said he was first induced to investigate the subject of his lecture as bearing on the structure of meteoric iron. Little or nothing was known of the minute structure of irons and steels, and it was requisite to devise appropriate methods to ascertain their intimate molecular constitution, and also to invent special means of illumination to enable the structure to be examined with a microscope. Much might be learned of the nature of iron and steel by artificial fractures, but these showed more the lines and planes of weakness, and the divisions between the constituent crystals, than the actual structure of the crystals and their relation to one another. It was, therefore, requisite to devise some means of ascertaining the exact structure of the metals, independent of any lines of weakness revealed by fractures. This was accomplished in the following manner. Thin flat portions of iron or steel were fixed on glass, and the upper surface filed flat and afterwards ground to a perfectly even surface on fine whetstones, so as to get an even section without any tearing or burnishing of the surface. Afterwards this upper surface was polished in such a manner as to leave the most minute portions of the metal quite undisturbed, and the surface entirely free from polished grooves or scratches. Great care should be used to avoid any irregular results

* Lecture delivered at Firth College, Sheffield.

which might be due to finger-marks or anything else which would disturb the subsequent process. This carefully polished portion of iron or steel was placed in very dilute nitric acid, and its action carefully watched. After remaining in the acid a short time the section was taken out and examined under water in a glass trough; and if the structure had not been sufficiently developed the section was again treated with the dilute acid. As soon as it was thought that the etching was in every respect satisfactory the specimen was quickly washed, dried, and a portion of thin glass mounted over it with Canada balsam. When all these processes had been carried out in a perfect manner such preparations might be studied with a magnifying power of several hundred linear. For this purpose, however, special illuminators were required, since the objects were, of course, opaque, and must be examined either by oblique surface illumination, or by a peculiar direct surface illumination first applied to this subject. The development of the structure by means of weak acid was due to the fact that some of the constituents were not acted upon at all, and others in varying degrees. Portions of slag or cinder remained in their original state, and were seen as black specks or patches of varying size and shape. Some constituents of iron and steel remained perfectly bright and brilliant, whilst others became coated to a varying extent with a more or less brown substance, so as to show the outline of the individual crystals very perfectly. Other constituents were so acted upon as to develop a very close grooved structure, which gave rise to very varying colors of exquisite tint and brilliancy. Thus, by difference of color or other characteristics, the outline of the individual crystals and their own intimate structure were shown to great perfection. Some of the objects prepared in this way might be reckoned amongst the most beautiful microscopical objects that could be seen, and no one who had not examined them would guess their nature, as some looked far more like organic bodies than anything one was accustomed to fancy characteristic of iron or steel. Other specimens were studied in a different way. The surface was polished in a less perfect manner, and the action of acid continued much longer, so as to produce sufficient difference in the relief in the case of the different constituents of the iron and the steel to enable prints to be taken from them as from woodcuts. In the case of some varieties the results thus obtained were exceedingly gratifying. These studies, Dr. Sorby went on to say, were carried out by him many years age, since which time, his attention had been devoted to other subjects, but he was convinced that a very great deal still remained to be learned. In fact, to thoroughly determine the exact nature of all the constituents seen in the specimens would involve many years of careful chemical and microscopical investigation; since, though many of them differed very greatly in microscopical and physical character, their size was so small that it would be difficult or impossible to separate them in such a manner as to determine their chemical constitution, and it would be requisite to ascertain their true nature by careful induction from facts observed under special circumstances. As far as could be learned from the careful use of the microscope, various kinds of iron and steel contained at least seven well-marked constituents. In the first place, there was pure iron, and what were probably three well-marked compounds of iron, with varying amounts of carbon or other substances met with in small quantities in different sorts of iron and steel; portions of included slag, well marked crystals of graphite, and small crystal, which might be silicon. The lecturer then proceeded to exhibit by means of the oxhydrogen lamp a considerable number of illustrations of the structure of various kinds of artificial iron and steel, some being photographed by Mr. Charles Hoole direct from the preparations, others from drawings by the author, and others from nature prints, made by the process already described. Commencing with various kinds of cast iron, it was shown that their structure was sometimes chiefly modified by the presence of crystalline plates of graphite, over which was deposited what was probably free iron, and the interspaces filled by what were probably two distinct compounds of iron and carbon, in other cases the structure was mainly dependent on the crystallization

of the iron itself, the graphite being thrown off towards the close of the process. In the case of white refined iron the principal constituent probably was an intensely hard white refined iron with much carbon, associated with which were one or more of the other compounds of iron and carbon present in grey iron. The microscopical structure of this white iron was exceedingly curious and beautiful. The next illustrations were of various kinds of wrought iron. The hammered bloom was shown to consist of an irregular mixture of crystals of iron and portions of slag. When rolled out into a bar those portions of slag not squeezed out were thrown out into long threads, but the crystals of iron seen in the bar were not the original crystals of the bloom, but fresh crystals formed on the cooling of the bar, since they exhibited little or no tendency to elongation in the line of the length of the bar, as would occur if the original crystals were drawn out by the process of rolling. The fiber seen on fracturing such specimens of wrought iron was mainly due to the elongation which occurred during the fracture, and was not a characteristic of the unaltered iron. In connection with this, illustrations were shown of the structure of armor plates, of welded joints, and all of those kinds of iron which are employed in the manufacture of steel by the cementing process. The change of structure produced by this cementing process was, the lecturer pointed out, very striking, the most characteristic feature being the development of a net-work of flat crystals of an intensely hard compound of iron and carbon, scarcely acted upon at all by dilute acid, so that the rest of the steel may be dissolved away, and this compound left in sufficient relief for exquisite prints to be taken as from a woodcut. Numerous illustrations thus taken direct from the iron and steel were exhibited with a lantern; and few microscopical objects are more beautiful than some of the preparations of this cemented blister steel, since, when specimens are prepared in the manner already described, some of the constituents gave rise to the most exquisitely beautiful colors by interference of light. The difference between the structure of the outside of the converted bars where this hard compound of iron and carbon had been developed and of the interior of the bar was shown to be very great, this latter being mainly due to re crystallization of the original iron. Ingots of cast steel produced by melting such blister steel had a totally different structure, which depended, in the first place, on large crystals, and in the second place, on the minute microscopical structure of these crystals. The principal difference between the structure of such an ingot and that of hammered bars was that the whole mass was made more uniform and the grain very much finer. This was still more the case when the hammered steel was hardened, in which case the constituent crystals were so small that it was very difficult to learn much about them by microscopical study. The structure of Bessemer steel ingots was materially different from that of the varieties of steel containing more carbon, and though of coarser grain, closely approached the structure of some varieties of Swedish irons. This structure, upon hammering, was greatly altered and became of finer grain and more uniform. In conclusion, the lecturer exhibited several illustrations of the structure of meteoric iron. This differed so much from that of most varieties of artificial iron that it was a long time before any point of similarity could be discovered. Alloys of iron and nickel of the same composition as meteoric iron were melted and slowly cooled, but nothing at all resembling the structure of meteoric iron was obtained. At length it was found that the closest approach to this structure was in the case of iron which has been kept for a long time at a high temperature, but not actually melted, under which conditions some varieties of iron containing little carbon crystallized in large crystals, having some of the important characteristics of meteoric iron, whilst iron containing a certain amount of carbon crystallized in a manner imperfectly resembling the very perfect crystallization of meteoric irons, only that in these artificial preparations there was crystallization of varying compounds of iron and carbons, whereas in the meteoric iron there were varying compounds of iron and nickel. The inference to be drawn from these facts was probably that meteoric iron crystallized very slowly at a temperature below fusion.

DEEP SEA SOUNDING.

By LIEUTENANT COMMANDER THEODORE F. JEWELL, U. S. N.

Record of the United States Naval Institute.

It is within the last thirty years only that any material addition has been made to our information as to what lies beneath the surface of the sea. For centuries men had speculated as to the depth of the ocean, but so far as accurate knowledge is concerned, they were completely ignorant. It was long held for instance, that analogy indicated that the deepest parts of the ocean were not deeper than the height of the highest mountains. But early experiments with long lines and heavy weights, seemed to show that the ocean's depths were unfathomable. Indeed, many exceedingly intelligent persons, among whom I may cite the late Vice-Admiral Fitzroy, of the Royal Navy, were of the opinion that a lead could not be made to sink to the bottom of the deepest seas, on account of the increased density of the water under the enormous pressure;—notwithstanding the fact that water had been shown, long before, to be practically incompressible. Irrational conceptions, such as this, of the difficulties attending deep-sea soundings seem to have prevented the accomplishment of much that, otherwise, might have been done. Yet Scoresby, writing in 1817 or 1818, recognizes clearly that the principal difficulty is the uncertain intimation given when the lead strikes the bottom; and he even suggests that this could be remedied if some method could be devised for determining the tension of the sounding line throughout its descent. The earlier devices for sounding all involved the use of a heavy sinker, with a correspondingly large line, and while they answered very well in depths not exceeding a few hundred fathoms, beyond a thousand fathoms they were valueless. If a large line were used the sinker would not carry it rapidly and vertically downward, while a lighter line was incapable of drawing up its own weight along with that of the lead. With a large line no impulse was felt when the lead reached the bottom, and the line would go on running out by its own weight, coiling itself over the lead. Indeed, this would happen with any line, and, in most cases, any attempt to check it was attended by its parting. The record of deep sea soundings previous to 1850, is consequently not only meagre, but entirely untrustworthy. It was but a natural sequence to such uncertainty that all sorts of devices should have been resorted to in order to dispense with the use of a line as a measuring instrument. The impregnation of wood by water under the greatly increased pressure was one of these devices, but it was found that the impregnation was practically complete at three hundred fathoms. Ericsson invented a lead in which air was compressed by the pressure of the water, and by the amount of compression the depth was to be estimated, but the instrument failed at great depths. Explosions were resorted to, the velocity of sound in water to be the means of measurement, but alike unsuccessfully.

But great strides have been made in the last thirty years in the development of deep-sea work, and instead of being unable to sound at all, we can now not only sound in deep water, but can do so with ease, certainty, and astonishing rapidity. Among those to whom we owe this extraordinary advance, officers of our Navy stand in the foremost rank. I am, therefore, about to present a short and, necessarily, an imperfect account of the process of this development, confining myself generally to the achievements of our own countrymen.

In October, 1849, the schooner Taney, Lieut. J. C. Walsh commanding, sailed from New York equipped for deep sea research. Her arrangements for sounding seem to have been made with great care, but she was small and unseaworthy —a vessel of but one hundred tons, that could not keep at sea to complete her cruise. In this little vessel steel wire was first used as a sounding line. I do not know whether the idea of using wire was original with Walsh or not, but it is interesting to note that this, the first

attempt at the employment of this material with which the greatest feats of deep sounding have since been done, was by an American Naval officer.

The Taney was supplied with 14,300 fathoms of the "best English steel wire," in five sizes, Nos. 5, 7, 8, 10, and 13, Birmingham gauge. The wire was tested to one-third more strain than it was estimated would be brought upon it. I give Walsh's language in describing how it was marked and prepared for use. "Of this an extent of 7,000 fathoms, weighing eighteen hundred lbs. (the remainder, consisting of the smaller sizes, Nos. 10 and 13, being stowed away as spare wire), carefully measured and marked with small copper labels, was linked into one piece, and wound upon an iron cylinder 3 feet in length and 20 inches in diameter—the larger sizes being wound first so as to be uppermost in sounding. Two swivels were placed near the lead, and one at each thousand fathoms, to meet the danger of twisting off by the probable rotary motion in reeling up. The cylinder with the wire was fitted to a strong wooden frame, and machinery attached,—fly-wheel and pinions, to give power in reeling up. Four men at the cranks could reel up with ease, with the whole weight of the wire out. Iron friction bands, which proved of indispensable importance, were connected to regulate the running off the reel. One man, with his hand upon the lever of one of these friction bands, could preserve a uniform, safe velocity, checking or stopping the wire as required. The whole apparatus could be taken apart, and stowed away in pieces (being so large and massive, this was indispensable in so small a vessel as the Taney). When wanted for use the frame was put together and secured to the deck by iron clamps and bolts, near amidships, the reel hoisted up from below and shipped in its place; a fair leader was secured to the taffrail, being a thick oak plank, rigged out five feet over the stern, having an iron pulley, 18 inches in diameter, fitted in its outer end, and two sheet-iron fenders 3½ feet long, of semicircular shape fitted under it to guard the wire from getting a short nip in the drifting of the vessel. The wire was led aft from the reel, over the pulley which traversed freely in the fair leader, and passed between the fenders into the water."

The first trial of this apparatus was made on Nov. 15th, 1839. The sinker used was a ten pound lead, and attached to the wire was an instrument, weighing six pounds, invented by Maury for recording the depth descended by the sinker. The cast was made under the most favorable circumstances; the sea was smooth and there was hardly a breath of wind. The wire went down vertically, "preserving," says Walsh, "the exact plumb-line throughout the sounding." When fifty-seven hundred fathoms had run out, the wire broke at the reel, but from what cause we are not informed. It was probably owing to the imperfection of the method used in joining the different lengths of wire together.

Walsh considered that in this sounding he had proved that the depth of the ocean at the point where it was made (Lat. 31° 59′ N. Long. 58° 43′ W.), about four hundred miles east of Bermuda, was not less than fifty-seven hundred fathoms. This depth was marked upon the chart with the sign of "no bottom." After a few years, however, Maury caused the sounding to be marked "doubtful," and finally in 1857 it was erased from the chart. Walsh's mistake was in using a sinker of too little weight for such large wire. A weight of sixteen pounds became insignificant in comparison with that of the wire (which in water was more than two hundred pounds to the thousand fathoms) when two or three thousand fathoms had run out, and consequently the shock when the sinker touched the bottom was inappreciable. The depth at the point where the sounding was made is between twenty-five hundred and twenty-eight hundred fathoms.

Several other unsuccessful attempts were made by the Taney to sound with the wire, but in every instance the wire broke when about two thousand fathoms had run out. This material was, therefore, deemed unfitted for the purpose and its employment was discontinued. Lieut. M. F. Maury, who was very active in the study of the ocean at that time, came to the conclusion that no reliance was to be placed upon soundings made in great depths with wire. But

the experiments of Walsh incited the Navy to new exertions. Sounding twine was substituted for wire; instructions for its use were prepared and issued, and a supply of twine furnished to every vessel in commission. This twine was of two sizes—the smaller was capable of supporting a weight of seventy pounds, the larger would bear one hundred and fifty pounds. Ten thousand fathoms of the small twine and five thousand of the large was supplied to each ship. The sinkers to be used were one or more 32 pdr. shot. The smaller twine was to be employed in great depths where there was little probability of recovering the shot; the larger, when it was probable the sinker might be recovered.

Among the first to use the twine was the sloop-of-war Albany, Commander Platt, which proceeded to sea in 1850. The officers of the ship entered heartily into the work, but the experiments were conducted under the direct supervision of the First Lieutenant, Wm. Rogers Taylor. The twine was wound upon a "delicately constructed reel which would turn with as little friction as possible." It was at first thought that this light line, weighing but a pound to 160 fathoms, would cease running out when the shot struck the bottom, or that it would, at least, move so much more slowly that the instant could be determined. This supposition was afterwards proved in error, and consequently the first soundings, which, indeed, were little better than guesses by the feel of the line, were subsequently discredited. Many discouragements were encountered, but they persevered. The twine proved of bad quality, breaking frequently when two or three hundred fathoms had run out, and we find recorded as lost in making one cast, as many as eleven shot. The line was overhauled fathom by fathom; weak portions were weeded out; and so at the end of six months they had made thirty-six casts. During this period the Albany expended forty thousand fathoms of twine, and in no instance where the depth exceeded one hundred fathoms was the shot recovered. In December, 1851, the ship was supplied with new twine, which had been very carefully made. One thousand athoms of this weighed 8¾ pounds. It was overhauled as before, the lengths were reknotted and parts of it were waxed. Notwithstanding these precautions, the line continued to part, but still much good work was done, and the experience thus gained was of great value to those who were to follow. As a result of these soundings it was demonstrated that the line would not cease running out when the sinker touched the bottom and that the feel of the line was an uncertain indication as to whether the sinker was on the bottom or not. It was also found impossible to make a cast from the vessel in any except the calmest weather. It was shown that the waxed twine would go down more rapidly than when unwaxed; and it was found that the twine, although tested to 70 pounds, would not weigh a 32 pound shot, owing to weak places, and the frictional resistance of the water.

Near the close of the Albany's work Taylor adopted a suggestion of Maury's and noted the time of running out of each hundred fathoms. The result is remarkable as indicating for the first time a means of determining with some degree of accuracy the instant when the sinker touches the bottom. It was found that from the beginning of the cast the time of running out of each hundred fathoms gradually increased, so that if at any particular hundred fathoms the time interval was increased more than its due proportion it was an indication that bottom had been reached. This was, probably, the most important fact developed by this cruise. So important did it seem to those interested in deep sea soundings at that time that the method was immediately adopted, and in nearly all, if not all subsequent casts it has been used. In the recent Challenger expedition it was the only method adopted for determining when the sinker was at bottom. The neglect in observing this time interval properly or the failure to interpret it correctly led Lieut. J. P. Parker, of the Congress, to report, in 1852, a sounding of eighty-three hundred fathoms; Capt. Denham, of H. M. S. Herald, one of seven thousand seven hundred six fathoms, the same year; and Berryman, in 1853, one of sixty-six hundred fathoms. Denham did observe the time of running out of each five hundred fathoms, but an examination of his

record shows that the bottom was reached at between twenty-three hundred and twenty-eight hundred fathoms. Parker did not observe his time intervals regularly, nor did Berryman. Maury estimates that Parker reached bottom in about twenty-eight hundred fathoms, and Berryman was afterwards satisfied that his sounding was incorrect.

Such was the state of affairs when the first really successful deep sea sounding expedition was organized. This was in 1852, in the Brig Dolphin, under the command of Lieut. S. P. Lee. Profiting by the experience of the Albany, Lee caused his sounding twine to be carefully examined as it was received, and rejected thousands of fathoms. Notwithstanding his supervision, much of the line was still defective, and of the first seventeen casts made, only the last was successful. Lee soon repeated the experience of Walsh and Taylor—only in the smoothest states of the sea, and in calm weather could good casts be made from the vessel. He, therefore, adopted the expedient of sounding from a boat, which by means of an oar on either side could be kept over the line. By doubling his line for the first two or three hundred fathoms he prevented its carrying away, and after that there was little trouble in obtaining quite reliable casts. Lee always noted his time intervals, though he did not always correctly interpret them. With a heavy sinker and a light line the shock when bottom is reached can usually be detected by those experienced in the work if proper care be used. But this method is uncertain and the officers of the Dolphin were frequently led into error by adopting it.

The problem of deep sea soundings was so far solved. The depth of the ocean could be determined at the expense of a 32 pound shot and a little inexpensive twine. The conditions necessary were a heavy, though not excessive, weight; a smooth, light line; some means of keeping the sounding vessel over the line; and an accurate record of the time required for running out of each fifty or one hundred fathoms.* The soundings of the Dolphin under Lee, and his successor Berryman, are universally recognized as the first ever made in the deep sea with any degree of accuracy.

But the subject was not allowed to rest at this point. Hitherto when a cast had been made the line had been cut or parted, so that no specimen of the bottom had been brought to the surface from any great depth. In order that a more complete knowledge of the bottom of the sea could be obtained, some contrivance for bringing up a sample of the soil became requisite. Heretofore this had been done only by weighing the sinker, to which some form of cup was attached, and, indeed, it had never been accomplished at depths exceeding a thousand fathoms. The sounding twine was not strong enough to weigh the shot, and some other device was necessary. At this stage, about 1854, Passed Midshipman Jno. M. Brooke appears with his apparatus, by which the shot could be detached when it reached the bottom and the instrument with a specimen of the bottom could be lifted again to the surface. I need not describe this invention, every one present is familiar with it. With some modification of its original form it remains to this day the most successful instrument of the kind ever invented. With it Berryman obtained specimens from depths exceeding two

Time of descent of each 100 fathoms after the first 300 fathoms.

Depth.	Interval	Depth.	Interval	Depth.	Interval.
Fath.	m. s.	Fath.	m. s.	Fath.	m. s.
400	1 41	1600	2 36	2800	3 08
500	1 49	1700	2 43	2900	3 09
600	1 51	1800	2 41	3000	3 10
700	1 58	1900	2 44	3100	3 10
800	2 02	2000	2 44	3200	3 10
900	2 10	2100	2 46	3300	3 12
1000	2 14	2200	2 51	3400	3 13
1100	2 26	2300	2 53	3500	3 20
1200	2 32	2400	2 59	3600	3 23
1300	2 32	2500	3 07	3700	3 28
1400	2 34	2600	3 07	3800	3 34
1500	2 42	2700	3 08		

This table shows that the line ran out for the first twenty-four hundred fathoms at a gradually increasing rate. After the twenty-five hundred fathom mark passed out, the rate at which the line ran out remained almost constant for nearly a thousand fathoms. This indicates either that the under surface current was carrying the line out, or that the weight of the line was doing so. In my opinion bottom was reached at about twenty-five hundred fathoms. Soundings of the Challenger, made near the reported position of this cast, show from twenty-five hundred to twenty-eight hundred fathoms. It is about three hundred a d thirty miles south and west from the position of Walsh's cast of fifty-seven hundred fathoms, and this circumstance may not have been without its influence in causing the error.

*To show the absolute necessity of this last requirement, when rope is used, I subjoin a record of a cast made by Lee in Lat. 26° 32′ N. Longitude, 60° 06′ W., and reported by him as 3825 fathoms.

thousand fathoms, and Belknap has repeatedly brought samples of the bottom from more than four thousand fathoms.

Brooke's invention gave a new impetus to the work of sounding. Equipped with this instrument Berryman again (in 1856) put to sea in the Arctic, and sounded all over the North Atlantic. One result of this cruise was the discovery of what has since been called "the telegraphic plateau." Berryman's soundings showed that between Newfoundland and Ireland, there existed a remarkably uniform depth of water not differing much from two thousand fathoms. As soon as this discovery was announced the project of connecting the two countries by a submarine telegraph cable was agitated. Berryman made twenty-four casts on a great circle between St. Johns and Valentia, with a view to determining the practicability of the scheme. He was followed, in 1857, by Capt. Dayman, of the Royal Navy, who in H. M. S. Cyclops, went over the same ground, making thirty-four casts. Dayman used Brooke's detaching apparatus with Massey's sounding machine, by which the depth was recorded. Brooke's original invention had already been modified by Berryman, who replaced the shot by a long leaden cylinder thus to diminish the resistance to the descent, and also adapted a valved cup to the end of the sounding rod. Capt. Dayman made similar modifications, and in addition replaced the rope slings of Brooke's original device by wire ones which were more readily detached. Massey's sounding recorder was found to be useful as a check upon the soundings, but for some reason, difficult to explain, it is not a reliable instrument in deep water.

From the time of Berryman's soundings in the Arctic, the United States Navy took but little part in deep sea work, for several years The English Navy, however, continued the work with great activity. Brooke's detaching arrangements was universally employed. Its use had gradually introduced the intervention of larger lines, but in 1860 we find H. M. S. Bulldog adopting the old plan of a cod line and an iron sinker, the line being cut at each cast. In this cruise, however, the soundings were usually repeated with a detaching sinker and larger line in order to obtain bottom specimens. Many efforts were made to invent a machine which would bring up larger bottom specimens, but the methods of sounding hardly varied. The use of steam rendered the lowering of boats unnecessary, as a steamer could be kept over the line when sounding. Small engines were also introduced for reeling in the line, thus diminishing greatly the labor of obtaining a cast.

The progress which had been made in deep sea sounding, in 1870, can best be indicated by a description of the process as practised on board the Porcupine in that year. This vessel, commanded by Staff-Commander Calver of the Royal Navy, was operating under the auspices of a committee of the Royal Society, with Prof. Wyville Thomson as scientific director. The following extract from Prof. Thomson's book, entitled "The Depths of the Sea," describing a cast made in two thousand four hundred and thirty-five fathoms presents the art of sounding in its most perfect development at that period.

"The Porcupine was provided with an admirable double cylinder donkey-engine of twelve horse-power (nominal) placed on the deck amidships with a couple of surging drums. This little engine was the comfort of our lives; nothing could exceed the steadiness of its working and the ease with which its speed could be regulated. During the whole expedition it brought in, with the ordinary drum, the line, whether sounding line or dredge rope, with almost any weight, at a uniform rate of a foot per second. Sometimes we put on a small drum for very hard work, gaining thereby additional power at some expense of speed.

"Two powerful derricks were rigged for sounding and dredging operations, one over the stern and one over the port bow. The bow derrick was the stronger and we usually found it the more convenient to dredge from. Sounding was most frequently carried on from the stern. Both derricks were provided with accumulators, accessory pieces of apparatus which we found of great value. The block through which the sounding-line or dredging-rope passed was not attached directly to the derrick, but to a rope which passed through an eye at the end of the spar, and was fixed to a 'bitt'

on the deck. On a bight of this rope, between the block and the bitt, an accumulator was lashed. This consists of thirty or forty or more of Hodge's vulcanized india-rubber springs fastened together at the two extremities, and kept free from one another by being passed through holes in two round wooden ends like the heads of churn staves. The loop of the rope is made long enough to permit the accumulator to stretch to double or treble its length, but it is arrested far within its breaking point. The accumulator is valuable in the first place as indicating roughly the amount of strain upon the line; and in order that it may do so with some degree of accuracy it is so arranged as to play along the derrick, which is graduated from trial to the number of cwts. of strain indicated by the greater or less extension of the accumulator; but its more important function is to take off the suddenness of the strain on the line when the vessel is pitching. The friction of one or two miles of cord in the water is so great as to prevent its yielding freely to a sudden jerk such as that given to the attached end when the vessel rises to a sea, and the line is apt to snap. A letting-go frame, a board with a slit through which the free end of the sounding machine passed, and which supported the weights while the instrument was being prepared, was fitted under the stern derrick. The sounding instrument was the 'Hydra' weighted with three hundred and thirty-six lbs. The sounding line was wound amidships just abaft the donkey engine on a large, strong reel, its revolutions commanded by a brake. The reel held about four thousand fathoms of medium No. 2 line of the best Italian hemp, the No. of threads 18, the weight per hundred fathoms 12 lbs. 8 oz, the circumference 0.8 inch, and the breaking strain, dry, one thousand four hundred and two lbs. soaked a day, one thousand two hundred and eleven lbs., marked for fifty, one hundred and one thousand fathoms.

"The weather was remarkably clear and fine; the wind from north-west, force = 4; the sea moderate, with a slight swell from the north-west. We were in Lat. 47° 38′ N., long., 12° 08′ W., at the mouth of the Bay of Biscay. The sounding instrument, with two Miller-Casella thermometers and a water bottle attached a fathom or two above it, was cast off at the letting go frame at 2h. 44m. 20s, P. M. The line was run off by hand from the reel and given to the weight as fast as it would take it, so that there might not be the slightest check or strain."

(Here follows a table showing the time of running out of each one hundred fathoms, the time intervals varying from 45 seconds for the first hundred to 1 m. 52 sec. for the last.)

"In this case," continues Prof. Thomson, "the timing was only valuable as corroborating other evidence of the accuracy of the sounding, for even at this great depth, nearly three miles, the shock of the arrest of the weight at the bottom was distinctly perceptible to the commander, who passed the line through his hand during the descent. This was probably the deepest sounding which had been taken up to that time which was perfectly reliable. It was taken under unusually favorable conditions of weather, with the most perfect appliances, and with consummate skill. The whole time occupied in the descent was 33 minutes 35 seconds; and in heaving up 2 hours 2 minutes. The cylinder of the sounding apparatus came up filled with fine, grey Atlantic ooze."

This was the sum of our experience in deep sea sounding when, in 1873, the Tuscarora, Comdr. Geo. E. Belknap, was ordered to prepare for soundings in the Pacific Ocean, a field hitherto unexplored. The object of the Tuscarora's cruise was, primarily, to determine a practicable route for a submarine cable to connect the United States and Japan. The original plan comprehended a line of soundings on a great circle, as nearly as might be, from Cape Flattery, Washington Territory, to No-Sima, at the entrance of Yeddo Bay, Japan. Returning, a line was to be run from Cape No-Sima, by the Bonin Islands, to Honolulu, and thence to San Diego or San Francisco. But a short coal supply prevented the completion of the first line, and bad weather, due to the lateness of the season, rendered it advisable to return. On the passage back to San Francisco lines of soundings were run on and off shore to determine the conformation the bottom near the coast line. This was continued afterwards as far south as San Diego. The southern line was then

run, the ship returning by the great circle route to the northward. The results of this cruise, together with a description of the apparatus employed have been published by the Hydrographic Office, in a fully illustrated volume, to which you are referred for more complete information.

The Tuscarora was supplied with about fifty thousand fathoms of sounding lines. Of this, some fifty thousand fathoms was 1¼, 1½ and 1¾ inches Manilla rope, which had been treated with carbolic acid, after some patented process, with a view to its preservation. About five thousand fathoms was 1¼ inch whale line, and there was also four thousand or five thousand fathoms of Albacore line made of untarred hemp, ⅜ inch in circumference. Fitted on the forecastle was a steam reel and a dynamometer, for use with this rope. The want of some instrument for regulating and measuring the tension of the line in sounding had long been felt. As the line runs out the tension is continually increased by the weight of the increasing length overboard, until it is suddenly diminished by the sinker resting on the bottom. If then the tension could be measured at every instant, any sudden diminution of the strain would be an indication that bottom was reached. The dynamometer, designed by Passed-Assistant Engineer T. W. Rae, was for that purpose. It consisted essentially of two fixed pulleys, elevated several feet above the deck, midway between which was a third pulley attached to a rod which was capable of motion in a vertical direction, through guides which it traversed freely. The lower end of this rod, which passed through the deck, terminated in a piston, which worked in a cylinder filled with water. The sounding line passed from the drum of the steam reel over one of the fixed pulleys, under the movable pulley, which was thus made to ride upon the line, over the second fixed pulley, and thence, by means of a fair leader, over the ship's side. The rod attached to the movable pulley could be weighted and by means of a scale behind the rod the strain upon the line could be determined. By gradually increasing the weights on the rod as the line ran out, it was intended to keep such a strain on the line, that it would cease running, or nearly so, when the sinker rested on the bottom. The object of the piston in the cylinder filled with water was to prevent violent motion of the rod, when the ship was rising or falling with the sea. This machine, although constructed upon perfect mechanical principles, and notwithstanding the fact that similar instruments are used with accuracy in the laying of submarine cables, gave hardly satisfactory results. It was found that the oscillations of the rod were violent and incontrollable when there was any rolling motion, and it was impossible to estimate with any degree of accuracy the tension of the line. It is possible that a more thorough acquaintance with this apparatus would make it valuable in sounding with rope. By an unfortunate accident it was not properly arranged in the Tuscarora, and hence did not obtain a very thorough test of its value, before the soundings with wire become so successful that its employment was unnecessary.

We have seen with what success the attempt to sound with wire had been attended in the Taney. Walsh's failure, together with that of other efforts made at about the same time, led to the conclusion that wire soundings were impracticable. Nevertheless that material offered great advantages and possessed the very qualities which experience had shown to be desirable in a sounding line. The small, smooth wire meeting with but little resistance from the water in descending, would sink rapidly, nor would it be deflected to any great extent by submarine currents; it would require a sinker of comparatively little weight; it could be made sufficiently strong to bear any ordinary strain; it was compact and portable, and would occupy but little room on board ship, a consideration which only those who have been embarked with great quantities of sounding rope, can properly appreciate. There were those, therefore, who did not accept the verdict rendered upon the evidence of former trials. All that was required to render the method practicable, it was maintained, was some contrivance for so regulating the strain upon the wire, that when the sinker reached the bottom, the wire should no longer run out, or should do so with such a diminished

velocity as to be readily perceptible. This was the object of a machine invented by Sir Wm. Thomson, in 1872, for sounding with steel piano wire.

One of these machines, with a supply of wire, was furnished to the Tuscarora. At that time but a single cast had been made with the apparatus, and that by the inventor, who sounded in twenty-seven hundred fathoms from a schooner yacht in the Bay of Biscay. The cast had not been satisfactory; the reel was crushed in the operation, and it was with great difficulty that the wire was hauled in. The original machine differed but little from that furnished to the Tuscarora, so that the method was almost absolutely untried when it was placed in Capt. Belknap's hands for experiment. The weight of opinion both in this country and in England was against the method. When the Challenger was fitting out in 1872, it was reported that she would be furnished with the machine. But she went to sea without it, the reason being, according to Sir Wm. Thomson, that "innovation is very distasteful to sailors." Prof. Wyville Thomson, who was the director of the scientific staff of the Challenger expedition, in a paper read before the Asiatic Society of Japan, at Yokohama, said, "When we started from England this wire had only been tried once. . . . I had been some years at sea and my colleagues were all sailors, so we had great sympathy with hemp." According to Sir William, the British Admiralty would not try the wire method because it was new. He states that he received a semi-official letter to the effect: "When you have perfected your apparatus we may be willing to give it a trial."

Whether or not it was sailor's prejudice that opposed the use of wire in this country, it is certain that few had any faith in its success. Fortunately, however, among the few, was the Chief of the Bureau of Navigation, Commodore Ammen. It was his determination in the matter that enabled the Tuscarora to put the method to the test. He facilitated the preparation of the ship in every way; he ordered Capt Belknap to make an experimental cruise to detect defects in his apparatus, which was the foundation of future success; and it is to his aid and counsel and constant interest throughout the work, supplemented by the ingenuity of Capt. Belknap, that the Navy owes its prestige in having made wire soundings practicable.

Thomson's machine as furnished to the Tuscarora, consisted of a reel for holding the wire, and an arrangement for regulating and measuring its tension. The reel was a hollow cylinder of galvanized sheet iron, with an iron axle passing through its center, and soldered to its sides. The drum of the reel was about six feet in circumference and three inches long. The ends extended two inches beyond the circumference of the drum, thus forming an annular space two inches deep and three inches wide in which the wire was wound. To one side of the drum was fixed a projecting ring of galvanized sheet iron, which formed with the side of the reel a V shaped groove in which an endless rope passed. The axle of the reel was a small iron shaft, six or eight inches long, which revolved in bearings on two iron standards, bolted to a plank of hard wood 3½ ft. long, 15 inches wide and 2½ inches thick. The shaft carried on one side of the drum an endless screw, which gave motion to a train of wheel work by which the revolutions of the reel were counted; on the other side was attached a ratchet in which worked a pawl by which the revolutions of the reel was prevented when necessary. Both ends of the shaft projected beyond its bearings and were squared, so that cranks might be applied in putting the wire on the reel.

The arrangement for controlling the tension of the wire consisted of a grooved friction wheel of iron ten inches in diameter, the groove being wide enough to carry two parts of the endless rope passing around the reel. It was capable o motion in a vertical plane, in an iron crotch fixed to the bed of the machine, but was prevented from turning by a cord passing from the lower part of its circumference to the dynamometer. The friction wheel was connected with the reel, when the wire was running out, by means of an endless rope of 9 thread untarred hemp, which passed around the V groove of the reel, then up, over, and once around the friction wheel, and then around a pulley placed several feet in the rear of the reel. This pulley

was attached to a small tackle by means of which the endless rope could be made more or less taut, thus increasing or diminishing the resistance of the friction wheel The tackle was shortly afterwards replaced by a pendant rove through a tail block, and carrying hooks to which weights could be attached, an arrangement of much greater utility.

The dynamometer was an instrument similar to one form of the spring balance, secured to the bed of the machine. The force exerted upon it, which depended upon the tension of the wire, was indicated in pounds, by a pointer which moved over a graduated scale. For this dynamometer was afterward substituted an ordinary spiral spring balance, which gave more satisfactory results.

The wire furnished for this machine was steel piano wire, No. 22, B. W. G., of the best English make. Its weight in air was about 14 lbs., and in water 12 lbs., to the thousand fathoms. It would support a weight of 230 lbs. It was supplied in lengths of from two hundred to four hundred fathoms which were spliced together on board ship. One great objection which had been urged against the use of wire was the impossibility of making the splices strong enough. The method of splicing adopted on board the Tuscarora was to lap the ends about two feet, solder one end and lay the other end up in turns of about an inch in length until it was all expended, when the second end was soldered. The two parts of the splice were also soldered together at intermediate points, and the whole splice served with well waxed twine. The splice thus formed was very strong, not one of them having broken down during the cruise. The wire was received packed in sperm oil in cases of sheet tin; it was kept in these cases covered with oil until wound upon the reel for use. The operation of winding was one requiring the utmost care, the wire having a great tendency to kink. When, by accident, a kink did occur it was found best, as a rule, to break the wire and splice the ends. In winding, the coil of wire was taken from the oil and slipped over the end of a wooden reel, from which it was wound on the sounding reel, being kept hand taut all the time. It was carefully measured as it was wound, the lengths between the splices, as well as the number of revolutions of the drum, being noted at each splice. These were recorded in a note-book and by them the depth was determined when a cast was made. The reel held readily between four thousand and five thousand fathoms, and this quantity was usually wound upon it. When it was all on, to the free end of the wire was attached a small grommet made of $1\frac{1}{2}$ or 2 inch rope. The grommet was secured by sticking the end of the wire between the strands of the rope and then taking several round turns against the lay, the whole being finally served. A piece of cod line attached to the grommet and tied around the reel prevented the wire from unwinding.

The required length of wire being wound, the reel was unshipped and placed in a galvanized iron tank containing a solution of caustic soda, which served to protect the wire from rust. The philosophy of this method of preservation, as explained by Sir Wm. Thomson, is as follows: "The preserving effect of alkali upon steel is well known to chemists. It seems to be due to the alkali neutralizing the carbonic acid in the water, for the presence of carbonic acid in water is the great cause of iron being corroded. The fact is well established that iron will remain perfectly bright in sea-water rendered alkaline by a little quicklime. Caustic soda is a more sure material, because with it we can make more certain that the water is really alkaline. * * * All that is necessary in order to make sure that the pickle will be a thorough preserver of the wire is that it should be found to be alkaline when tested with the ordinary litmus test paper."

I give Sir William's language as nearly as may be because I do not think he estimates at its true value the action by which the wire is preserved from rust. In the Tuscarora the lye in which we kept the wire was much more strongly alkaline than he says is necessary. It was found, however, that while the wire was perfectly preserved, the caustic soda solution attacked the zinc of the galvanized iron of which the reel was made, as well as the soldered splices. Now zinc and iron, or iron and tin solder, when placed in contact in caustic soda

form a galvanic couple of which the zinc or the tin solder is the electro-positive element. A galvanic action is, therefore, set up by which the iron is preserved at the expense of the zinc or tin. It is probable that this action would be less with a more dilute solution of soda than with a stronger one, but still great enough to protect the sounding wire from rust. This, I think, is the true explanation of the preservative action. If so, it seems to me that a more suitable liquid than soda-lye might be substituted for it. Caustic-soda is a very disagreeable substance to use on board ship. It spoils the clothes and hurts the hands of those working with the wire; and the lye, washing over the sides of the tank in the rolling of the ship, kills the wood of the deck, and then running through the scuppers takes the paint off the ship's side. I am inclined to think that slightly acidulated fresh water, or even sea-water alone might be substituted for it with advantage. So serious has the objection to the use of soda become that it has been discarded in English vessels using the wire, and sperm oil is now used in its stead.

The Tuscarora's soundings were always made under steam and with the ship stern to wind. This was found to be the best method of laying the ship and it was resorted to even when the force of the wind was as great as 8. Usually the screw held the stern of the ship up to the wind, but when the bows showed a tendency to fall off to one side or the other, which was the case only when the wind was light, the jib being set with the sheet hauled flat aft effectually prevented it. After numerous experiments Capt. Belknap fixed upon the gangway as the most convenient point from which to sound. There the motion of the ship was least sensible, the wire was consequently more manageable, and accordingly nearly all the soundings were made there. A bridge across the deck was constructed, so arranged that it could be unshipped and stowed away in port. The machine was placed upon a slide so that it could be run out or in and compressors were applied to secure it at any point. The upper grating of the accommodation ladder being shipped, this slide was supported by a railing and securely lashed. The grating allowed room to the men who were handling the sinker, &c., and served to carry the wire well out from the ship's side.

In preparing to sound, the reel was placed on its bearings at the gangway, and the bed run out to the end of the slide. The endless rope was arranged as already described. To the grommet was hitched a piece of Albacore line, twenty-five fathoms in length, and this was also wound on the reel. The other end of the Albacore line carried a small iron rod six feet long, to which was seized the upper end of the swivel link of the Brooke detaching apparatus. The Albacore line was to prevent the wire itself from going to the bottom, thus avoiding all danger of kinking, and the rod was for the purpose of throwing the line clear of the specimen cylinder so as to prevent fouling. The sinker which was usually an 8 in. shot weighing 55 lbs., was placed on the apparatus for obtaining the bottom specimen, an invention of Capt. Belknap, shortly to be described. When all was ready the sinker was eased down by hand into the water, and a Miller-Cassella deep-sea thermometer, for obtaining the bottom temperature, was attached to the stray line above the iron rod. The stray line was then allowed to run out slowly until the grommet in the end of the wire was reached. To this was attached a lead weighing four pounds, which prevented the end of the wire from flying up and kinking when the sinker reached the bottom, as experience had shown it would sometimes do. Weights were now hooked to the pendant carrying the pulley around which the endless rope passed, and the wire was allowed to run out. When it had fairly started, the weight on the pendant was diminished so as to allow it to run more rapidly. The time the wire started was noted, as well as the instant at which the drum completed each hundred revolutions. When it was thought that the sinker was nearing the bottom, the weights on the pendant were increased in order to diminish the speed of the reel and to make sure that the instant of reaching the bottom should be properly indicated. This indication was usually unmistakeable; the pointer of the dynamometer would fly back on the scale, and, except

in very deep water, the revolution of the drum would almost instantly cease. The fact that bottom had been reached could be noted as well from the poop or the forecastle as at the gangway.

Bottom having been found, the cord holding the friction wheel was detached from the dynamometer so that the endless rope might be employed in reeling up the wire. The officer in charge of the sounding then laid hold of the endless rope and, unaided, hauled in a few fathoms to make sure that the shot had been slipped. If not, as was the case in but few instances in the early part of the cruise, fifty or sixty fathoms were reeled in and again let go, the second effect almost invariably detaching the shot. The reeling in was at first done by putting men on the bridge who hauled in hand over hand on the endless rope, an operation both tedious and laborious. For this arrangement was afterwards substituted a fly-wheel, carrying a grooved disk of wood from which a belt passed to the V groove of the reel. By turning the fly-wheel by means of long cranks, which could be manned by four or six men the wire was wound in much more rapidly and uniformly than by the old system. When reeling in the wire was guided fair on the reel by two petty officers, round sticks being used for the purpose.

In Brooke's original apparatus the device for obtaining specimens of the bottom consisted of a number of open quills placed in the lower end of the sounding rod. This arrangement, however, did not secure adequate samples, and the ingenuity of those engaged in deep sea work has ever since been directed toward its improvement. Berryman soon replaced the quills with a valved cup, which gave much better results. Many subsequent improvements in the form of the cup have been made, not a few of which have been by officers of the Navy. In the Challenger, the apparatus usually employed was the "Hydra," so called because it was invented on board an English surveying vessel of that name. The Hydra consists of a long cylinder of brass, closed at its lower end by a valve opening upwards. An iron piston-like rod, carrying a short arm projecting at right angles to the rod, is fitted into the upper end of the cylinder. Over the projecting arm is a curved steel spring, one end of which is fast to the rod, the other end movable. This may be pressed flat against the rod, a hole in the spring allowing the projecting arm to pass through it. The sinker is supported on the rod by a wire sling which passes over the arm and is kept there in opposition to the spring, by the weight of the sinker. When the sinker rests on the bottom the sling is pushed off the projecting arm by the spring, and the rod and cylinder are hauled up. The piston-like arrangement of the rod consists in closing the valve in the lower end of the cylinder.

This apparatus has been used very successfully in the Porcupine, and was highly thought of in the Challenger. Its weight, however, made it objectionable for use with wire, and it necessitated the use of a separate instrument, the water-bottle, when samples of the bottom water were required. In order to overcome these objections several new forms were devised by Capt. Belknap. Of these the most important and most successful are those designated as specimen cylinders, Nos. 1, 2 and 3. In each of these the Brooke plan of detaching is adhered to.

Cylinder No. 1 consists of two cylinders, and the inside diameter of the one being very slightly greater than the outside diameter of the other, so that the larger slides over the smaller with but little friction. The inside cylinder terminates in a cone, above which, on two opposite sides, are openings, by which the bottom mud or sand can enter. The upper end of this cylinder screws on the iron detaching rod. The outside cylinder travels on this rod by means of an opening in its top. It is long enough to go entirely through the shot used as a sinker, and, when in its lowest position, it covers the openings in the inner cylinder. A stud on one side prevents its slipping through the shot. The inner cylinder has in its upper part a chamber, to the top and bottom of which are fitted valves, opening upward, intended to enclose a specimen of the bottom water. In slinging the sinker this apparatus is passed through a hole in the shot, and a metal washer is put on, which by means of wire slings is suspended to the detaching arm of the rod. When the shot

rests on the bottom, the detaching arm falls, the outer cylinder slips down and retains whatever has entered the inside cylinder.

Cylinder No. 2 is of quite different design. To the lower end and inside of the cylinder is fitted a hollow frustum of a cone, its base downward. The upper end of the cone is closed by a valve, kept in place by its own weight in addition to a light spiral spring. Into the bottom of the valve is screwed a plunger extending beyond the end of the cylinder. When the shot strikes the bottom, the valve is forced open and the mud or sand enters. The valve then closes, retaining the specimen. This cylinder is also fitted with a water space.

Cylinder No. 3 consists of an auger-shaped piece of iron, over which slides a brass cylinder. The brass cylinder is kept up by a stud on its side, until the shot is detached. The auger-shaped iron engages the bottom specimen which is retained by the cylinder. In the use of this cylinder it was sometimes found that the metal washer which supported the shot was caught by the cylinder in falling, and prevented the shot from slipping off. This was remedied by lacing on the shot two wire grommets of smaller diameter than the shot, to which the slings were attached.

Cylinder No. 1 was most successful in soft bottom, at moderate depths. No. 2 brought up the best specimen in hard, sandy bottom, and was always good. No. 3 answered best at great depths, when the shot on striking the bottom was not moving with great velocity.

In depths less than two hundred fathoms the sinker used was an 8-inch shot, weighing about 55 lbs. In deeper water one or more lead castings, which fitted over the top of the shot, were added, thus increasing the weight to 70 or 75 lbs., and making the total weight sent down, including the specimen cylinder and the small safety lead, about 80 lbs.

The time required for a sounding with this apparatus is much less than that required for rope, even when the rope is reeled in by steam. The deepest sounding made by the Porcupine in 1869 and '70 was the one of twenty-four hundred and thirty-five fathoms, a description of which I have read. The sinker used on that occasion weighed 336 lbs. The time required for the descent of the line was 33½ minutes, and in hauling in, 2 hrs. 2 min., or 2 hrs. 35 minutes for the cast. In the Tuscarora we sounded in twenty-five hundred and sixty-five fathoms, the line running out in 31 minutes, and being reeled in in 40 minutes, the whole time occupied being but 1 hr. 11 min., a gain of 1 hr. and 24 minutes, although the depth was over a hundred fathoms greater. This is a very fair example of the speed with which a wire sounding could be made. A cast of three thousand fathoms usually required an hour and a half, and one in four thousand could be made in about two hours.

The Tuscarora ran lines of soundings aggregating sixteen thousand six hundred and twenty miles in length, and made in all four hundred and eighty-three casts, of which perhaps fifty, in depths generally less than five hundred fathoms, were made with rope. The depth found in one hundred and sixty of the casts was over two thousand fathoms; thirty-two were in depths of more than three thousand, and nine, in depths of more than four thousand fathoms. The greatest depth from which a bottom specimen was obtained was four thousand three hundred and fifty-six fathoms. The deepest sounding made was in four thousand six hundred and fifty-five fathoms, or 5¼ statute miles. The circumstances under which this cast was made were all the most favorable. The sea was smooth, the ship steady, and the up and down direction of the line was maintained throughout. The indication of the dynamometer that bottom had been reached was perfect. But the wire had been down four thousand fathoms three or four times on the preceding day, and it finally gave way under the strain when about four hundred fathoms had been reeled in. This was the fifth and last time during the cruise that the wire was lost.

The soundings with the wire were invariably made by the navigator of the ship, Lieut. Geo. A. Norris, and it was to his industry and intelligence that their success was in a large measure due. To Capt. Belknap, however, we owe the perfection which this method has attained. He was indefatigable in the

work and every cast was made under his direct supervision. Both Commodore Ammen and Sir Wm. Thomson were, naturally, much gratified at the results of the cruise. The latter has repeatedly publicly referred to the fact that while the British Admiralty hesitated, the American Navy perfected his apparatus in its own way. In his presidential address before the Section of Physics of the British Association, he speaks enthusiastically "of Commander Belknap and his great exploration of the Pacific depths by piano-forte wire, with imperfect apparatus supplied from Glasgow, out of which he forced a success in his own way, * * * and of the admirable official spirit which makes such men and such doings possible in the United States Naval service."

The great difficulty that was experienced in the Tuscarora, and which the resources of the ship could not overcome was the crushing of the reel, especially when the soundings were in deep water. The steel wire stretching under the strain to which it was subjected, was reeled tightly on the drum. The crushing force thus brought upon the drum by the elasticity of the wire was very great, and the drum, being made as light as possible in order to keep its inertia small, invariably broke down. A drum would stand, perhaps, a dozen casts in depths below two thousand fathoms, but when the depth was much greater the life of a drum was much less. The reels on board the Tuscarora were constantly in the hand of the tinsmith. They were strengthened by radical pieces of wood, placed inside of them, but the crushing went on almost as fast as though the wood were not there. Other devices were resorted to but unavailingly, and the only way of keeping at work was to shift the wire from reel to reel as they broke down. Capt. Belknap recommended a steel drum for deep soundings, but I do not know that any have been constructed. Sir Wm. Thomson has overcome this difficulty by the introduction of an auxiliary wheel in reeling in.

The bed of the machine, rests on two rails so that it can be run out or in, and clutches at either end of the bed keep it on the rails. The galvanized sheet iron reel is retained, and the soundings are made directly from it as before. Under the rails and midway between them are two pulleys, and one of which projects over the taffrail, supposing the sounding to be made from the stern as Prof. Thomson prefers. This outermost pulley is called the "castor pulley" from the manner in which it is mounted. Its bearings are in an oblique fork which turns about a horizontal axis, like the *castor* of a piece of furniture laid on its side. The other pulley is inboard. The two bearings of its axle are both on the same side of the pulley so that a turn or two of the wire can be taken around it. The ends of its axle are squared so that handles may be applied in reeling in, or a wheel having a sharp V groove can be shipped, carrying an endless rope by which the reeling in is done. When the sounding has been made the wire is stopped and the reel run in about five feet on the slide, until it is just over the auxiliary pulley. The wire is then led over the castor pulley and a turn, or two turns, are taken around the auxiliary pulley. The reeling in is performed by the auxiliary pulley which takes from two-thirds to nine-tenths of the strain off the wire before it reaches the sounding reel on which it is wound as the reeling in proceeds. The castor pulley, by turning about its horizontal axis, in case the ship drifts during the operation, prevents the wire from leaving the groove; it performs the same office if the ship is rolling heavily. Additional security is obtained by a guard to catch the wire if it should get off the pulley.

In the machine, as thus modified, the inventor has introduced a friction arrangement different from that used in the Tuscarora, and has abandoned the spring dynamometer. A rope fastened to the bed of the machine passes over the V groove of the reel and then over two small pulleys. To the end of this rope weights are applied. These are added as the wire runs out so that the friction brings the drum to rest as soon as the sinker touches the bottom. The rule is to apply a resistance greater by ten pounds than the weight of the wire out. This makes the moving force on the sinker ten pounds less than its weight in water and a sinker of thirty-five pounds is thought to be sufficient in depths less than three thousand fathoms.

The machine, as thus modified, was used in the cable ship Hooper in laying the submarine cables on the coast of Brazil; it was also used in the Faraday, in laying the direct Atlantic Cable. In these ships the sinker was always recovered, and Sir Wm. Thomson recommends that this should be done whenever the depth is not over three thousand five hundred or three thousand fathoms.

One difficulty remained to be overcome. When the ship is lifting in a heavy sea there are times when a very great strain is imposed upon the wire and when it would be dangerous to haul it in too fast. Prof. Jenkin, of the University of Edinburgh, who accompanied Sir Wm. Thomson in the cable laying expeditions, has invented an arrangement by which the hauling in may go on as rapidly as possible, without bringing more than a certain safe strain upon the wire. The wire will come in fast when the strain is easy, and not come in at all when it is too great. By this arrangement steam can be applied to reeling in the wire. I regret that I do not know the details of this exceedingly important device.

A most valuable and ingenious improvment in the Thomson sounding machine has been made by Lieut.-Commander Sigsbee, United States Navy, who has been engaged in sounding in the Coast Survey steamer Blake. In the Tuscarora, whenever the ship was rolling during a sounding, the revolution of the reel would almost cease when the ship rolled toward the wire; on the other hand when the roll was in the opposite direction the reel would move so rapidly that it would have thrown the wire off, if the motion had not been controlled. This was done by pressing with the hand on the endless rope, but this brought an undue strain upon the wire. Sigsbee's invention controls the motion of the machine automatically.

One either side of the bed of the machine, at its outer end, is an upright stanchion. Between the two stanchions is a horizontal bar which moves up and down in slots or scores in the uprights. A fixed cross-piece joins the upper ends of the two uprights, and the movable bar is connected with the cross-piece by two spiral springs. Attached to the under side of the movable bar is a pulley. To the strap of this pulley is secured one end of the brake rope. This is led around a small pulley, on the bed of the machine, then carried around the V groove of the sounding reel, and its end made fast to a small eye bolt. When there is no downward strain on the bar the springs cause the rope to press tightly against the reel, and keep it from revolving. The wire, instead of passing directly from the reel into the water, is led over the pulley attached to the movable bar. Consequently when considerable strain is on the wire the springs are stretched, the brake rope is slackened and the reel may turn freely. Any tendency of the reel to pay out the wire more rapidly than the sinker will take it, is attended by a diminution of the strain on the wire; the springs contract and the brake-rope is applied to the reel. The changes in the tension of the wire being thus self-regulating it is evident that no dangerous strain can be brought suddenly upon it.

Although not strictly within the scope of my paper, I have thought it well, at this point, to speak of the most recent application of the piano wire in sounding. I refer to its substitution for rope in sounding in shoaler water for the ordinary purposes of navigation. The uncertain process hitherto adopted, involving the necessity of stopping or heaving-to the ship, and depending upon the shrewdness in guessing of the quartermaster, is no longer necessary. In laying the telegraph cables on the coast of Brazil, Sir Wm. Thomson used the ordinary deep-sea machine in making approximate soundings in depths of from fifty to one hundred and fifty fathoms while the ship was running at the rate of four or five knots. The depths in these cases were arrived at by noting the length of wire out, and knowing the distance run by the ship while it was going out, and were therefore only approximations. He has, however, recently modified his apparatus so as to make it available for "flying soundings," and they have been made with great accuracy. The reel used for this purpose is of the same form as that for deep soundings, but it is only twelve inches in diameter and holds only three hundred or four hundred fathoms of wire. The brake arrangement differs somewhat in form

from that of the larger machine. As soon as bottom is reached, which is shown by a sudden decrease in the speed of the drum, a brake is applied and prevents any more wire running out. A counter is used to show the number of revolutions of the drum. The wire is reeled in by applying cranks, worked by two men, to the axle of the drum.

The sinker is a slender cylinder of iron, three feet long, weighing twenty-two pounds, attached to the wire by a fathom and a half of log line. The depth is determined by the compression of a column of air contained in a glass tube, closed at one end, two feet long and about three-eighths inch internal diameter. The tube is coated on the inside with a colored substance, which is discolored by contact with sea-water. The glass tube, to secure it from breaking, is inserted with its open end downward in a slightly larger tube of brass, made fast to the log-line above the sinker. As the sinker descends, the pressure of the water reduces the volume of the air in accordance with Boyle's law, the water rises in the tube, and the height to which it rises is marked by the discoloration of the chemical preparation within the tube. A graduated scale is applied to the tube by which the depth in fathoms is determined by the extent of the discoloration.

There is no reason why such an apparatus should not be carried on board every ship. The frame of the machine could be secured to the taff-rail when the ship is on soundings. "Two men," says Sir Wm. Thomson, "can take a cast with ease every quarter of an hour." It is not necessary to stop the vessel or heave her to in making a cast. The depth indicated by the tube depends only on the vertical distance descended and is independent of the inclination of the wire. Nor is it necessary to use the chemically prepared tube each time. If the speed of the ship be uniform, the reading of the counter when the tube is used will show the relation between the depth and the length of wire out, and the succeeding three or four casts could be estimated entirely by the counter. This apparatus has been used successfully in H. M. S. Minotaur when the ship was going ten knots, and t is reported that a sounding has been made with it from one of the White Star Steamers when the speed was sixteen knots.

The Tuscarora having demonstrated the feasibility of the wire method, the art of deep-sea sounding has been revolutionized. The days of rope soundings terminated last year with the cruise of the Challenger. With the exception of those made in that ship every deep sounding within the last three years has been made with wire. The inconsiderable labor necessary in making a caste, the short time required for the purpose, the accuracy of the result, are such that, henceforth, the bed of the ocean lies but at our hand. "International coöperation alone is necessary in order that the principal features of the bottom of the sea may be mapped out with almost the same accuracy as that with which the geographer now depicts the land surface." In every step toward this wonderful result—wonderful, when we consider the few years in which it has been accomplished—those to whom we are united by ties of official brotherhood have largely contributed. It is to preserve the recollection of this fact that I present this imperfect record of their achievements.

The range of the changes of level in the rivers of Russia in Europe has become, since 1876, the subject of accurate measurements, and M. Tillo has just published an interesting paper on this subject, being the result of measurements made at eighty different places. The highest range is reached by the Oka at Kaluga, the difference between the highest and lowest levels being as much as 45 ft.; the average range for the same river from its source to its mouth being 32.2 ft.; the average for the Volga from its source to its mouth is 33.6 ft., 30.1 ft. for the Kama, 25.2 ft. for the Duna, and 23.1 ft. for the Don. For all other rivers the range is less than 20 ft. Of course this range diminishes very much towards the mouth of each river; but still it reaches 12 ft. for the Volga at Astrakhan, and 9 ft. for the Duna at Riga. The highest range observed in the lakes of Northern Russia was only 2.1 ft. A map prepared by M. Tillo shows the distribution of hydrometrical stations on Russian rivers, their numbers having been increased in 1880 to 341 stations.

SILT MOVEMENT BY THE MISSISSIPPI—ITS VOLUME, CAUSE AND CONDITIONS.

By ROBT. E. McMATH, Member of the Engineers' Club of St. Louis.

Journal of the Association of Engineering Societies.

THE volume of solid matter borne by rivers, the mode of its conveyance and the conditions of deposit and removal are topics of interest to the engineer who has to do with works at the margin or in the bed of silt-conveying streams. These topics have special interest at the present time, because of their bearing upon the improvement of the Mississippi.

I propose to state in brief the result of my personal study or observations made under my immediate charge, as assistant to Gen. J. H. Simpson, U. S. Engineers, adding data from other sources when needed to complete or support a conclusion.

The observations were made under favorable conditions, using apparatus of improved design. The locality chosen was a few hundred feet above the foot of Grand avenue, St. Louis, where the width and sectional area of the river approximated a mean value.

Samples of 100 grammes each were taken daily, 1 foot below surface, at mid depth, and 1 foot above bottom, at positions dividing the width into 8 parts—making 23 daily samples. The daily samples from each position were combined for periods of 6 and 3 days, as shown in the table on the opposite page.

Perhaps a better apprehension of the amount of turpidity may be gained by remembering that 1,000 parts in 1,000,000 by weight is very nearly one ounce of solid mtter to a cubic foot of water. To reduce to ounces per cubic foot cut off three decimal places in the column headed "sediment."

Specimens were weighed dry after continued exposure to a temperature of 180° F. A cubic yard of compacted sediment was taken at 1.6 tons (3,200 lbs.).

During the 87 days between March 31 and June 25, 1879, 62,363,009 cubic yards of solid matter passed the observation station in suspension. A quantity sufficient to cover a square mile to a depth of $60\frac{4}{10}$ feet.

Observations were made the same season by Maj. C. R. Suter to determine the volume of solid matter borne by the Missouri River near St. Charles. The general results have been reported as

		Cubic yards per day.
Mean volume	for the year 1879....	475,457
" "	" June and July..	1,755,423
Maximum"	on July 3	4,113,600

The stage at St. Louis for June ranged from 13.2 to 21.0; mean, 16.6. For July the range was 21.0 to 16.1; mean, 18.0. The "danger line," or beginning of damaging overflow, corresponds to a stage of 30 feet. Extreme known flood to 41.38.

The observations, therefore, do not include a flood or even what would be called a high stage.

Observations for 250 days at Fulton, Tenn., made by the Mississippi River Commission, gave a maximum of 3,232,209 cubic yards on July 10, 1880. The aggregate for the period January 1 to October 8, 1880, was 217,728,125 cubic yards, or sufficient to cover a square mile to the depth of $210\frac{8}{10}$ feet. The minimum discharge for any day at Fulton was 136,458 cubic yards, confirming the minimum observed at St. Louis.

The comparatively small volume of sediment at low stages is of itself sufficient proof that important changes, by way of deposit in the bed, cannot occur at low stages except as a result of local movements.

Considering the moving mass as among the material resources of the engineer on one hand, or as a possible opposing force on the other, it appears from the foregoing figures that at one season the quantity is scant and the forces feeble, at another the quantity is enormous and the forces overpowering unless skillfully handled. It is readily seen how necessary to an intelligent dealing with the river is a thorough knowledge of how, when and why the silt movement occurs.

If one examines the material found in suspension he will discover that a consid-

Dates.	Stage.		Sediment Parts in 1,000,000.					Mean velocity of river. Ft. per sec.	Quantity of sediment in cubic yds. Per day.
			Mean of all stations.			Near sides.			
	Range.	Mean.	Surface.	Mid. depth.	Bottom.	Mo ordinate 281'.	Ill. ordinate 1291'.		
	Ft.	Ft.							
1879.	11.0								
March 31–April 5.	12.5	11.9	533	493	581	1003	270	4.34	138,182
	12.3								
April 7 " 12.	17.3	14.5	1998	2363	2398	3100	1173	4.58	782,434
	18.0								
" 14 " 19.	14.9	16.5	3601	3731	3809	4200	3400	6.13	1,555,646
	14.0								
" 21 " 26.	12.9	13.4	1581	1657	1814	2190	1298	5.20	449,985
	12.6								
" 28 " 30.	12.3	12.5	1509	1568	1577	2100	1017	4.93	396,585
	12.2								
May 1–May 3.	11.2	12.1	1351	1377	1378	1825	951	4.56	349,903
	11.8								
" 5 " 7.	11 6	11.7	996	1066	1096	1690	560	4.14	282,892
	11.8								
" 8 " 10.	12.1	11.95	1332	1378	1456	1951	945	4.00	343,475
	12.2								
" 12 " 14.	11.8	12.0	2014	2118	2166	2771	1605	4.14	508,521
	11.75								
" 15 " 17.	11.8	11.8	2267	2449	2467	2948	1973	4.07	559,457
	11.5								
" 19 " 21.	11.3	11.4	1408	1516	1639	2362	1082	4.12	360,335
	11.4								
" 23 " 24.	11.5	11.25	1556	1685	1793	2432	1297	4.11	391,404
	11.5								
" 26 " 28.	12.0	11.7	1236	1311	1646	2302	992	4.17	357,160
	12.3								
" 29 " 31.	12.9	12.6	1055	1232	1250	2408	653	4.55	378,960
	13.4								
June 2–June 4.	15.3	14.3	1274	1429	1530	3188	652	5.52	560,681
	15.5								
" 5 " 7.	15.9	15.7	1863	2213	2327	3453	1210	5 81	932,666
	16.5								
" 9 " 11.	16.0	16.3	1567	1791	2056	3103	1097	6.23	886,090
	16.0								
" 12 " 14.	16 1	16.0	1553	1765	1948	2878	1165	5.86	839,490
	16.3								
" 16 " 18.	16.4	16.3	2281	2596	2752	3245	2057	6.31	1,134,475
	17.0								
" 19 " 21.	17.7	17.3	3347	3651	3818	3882	3693	6.93	1,656,553
	17.9								
" 23 " 25.	17.5	17.8	4477	4796	4963	4425	4883	7.21	1,983,467

erable proportion is in a state of subdivision so minute that it separates from the water very slowly. The experiments of Col. Flad made in 1865 are in point. He found that of 1,000 parts of matter in suspension at the beginning of the experiment,

944.50 parts settled to the bottom in 1st 24 hrs.
22 35 " " " " " 2d 24 hrs.
2.92 " " " " " " 48 hrs.
30.23 remained in suspension at end of 96 hrs.

The practical problem of river engineering has little to do with this very fine matter. Its presence and transport is analagous to that of impalpable dust in the air and requires no comment.

Without drawing arbitrary lines of classification as to weight or size of particles transported, which may vary from impalpable dust to the rock fragment, occasionally rolled over by the current, we can recognize three grades or species of movement and material.

First. Some of the traveling material never loses contact with the bottom.

Second. Material which, though heavy and in grains of notable size, is detached for a time by some energetic impulse and describes a longer or shorter free path, moving in or with the surrounding water.

Third. Material which, once mingled with the water, remains in continuous suspension until it reaches the sea.

No measure of the first grade movement has yet been satisfactorily made, being so far as manifested by progress of sand waves, undistinguishable from movements of the second grade. It is probable that the ordinary impression of its amount is an exaggeration.

The muddy appearance of the river is mostly due to the fine or third-grade matter. The proportion of such matter present is in close accordance with the appearance. But appearances give slight clue to the more important second-grade movements below the surface, which are manifested to the ordinary observer only by their results in bars deposited or removed.

Recurring to the figures stated, at first thought the transportation of so great a mass seems to present an important dynamical question.

Mass transported is work done, and work absorbs force. Water is matter, and its transport in a running stream is work. We say readily that the force is gravity, the body falling.

A chip floating with the current is a solid body being transported; it is, however, but a representative of the volume of water it displaces, and its motion is due to the same cause. It is not moved by, but moves with the water. So far from its transport being at the expense of the stream, the quantity of motion (MV.) is increased by its presence. The like may be said if the solid be of the same density as water, and immersed at any point of the depth. The effect upon MV. is the true test; therefore, if a pebble be thrown into a stream the quantity of motion will be increased or diminished, as the direction of its projection is with or against the current. It is thus seen that transportation in a running stream is an incident to the state of suspension, immersion or flotation. The inquiry that interests us turns to the cause of suspension; or how bodies heavier than water can be sustained clear of the bottom for an indefinite time. If heavy bodies, when cast into the stream, always reached the bottom after an interval of time and distance, and remained there, the possible movement past any station would be measured by the contributions of matter from outside sources within a very moderate distance. But, since the quantity moving and the supply from local sources do not correspond, we are compelled to conclude, that a force exists within the stream capable of resisting the descent of injected solid bodies, so that transport of contributions is continuous, or else the force is capable of the more serious work of detaching such solids from the bed, or banks, and projecting them upwards even to the surface, or laterally to considerable distances. The facts demonstrate the existence of such an upward and lateral force.

Three hypotheses have been suggested to account for or define the force.

First Hypothesis. It is due to the differences of velocity with which contiguous layers or fillets of fluid move, the submerged or floating body being impelled toward the fillet of greatest velocity.

Second Hypothesis. It is due to a general vertical component of the stream's motion, by which the water at the bottom is continually being brought to the surface.

Third Hypothesis. The power of erosion and suspension is due to the irregular movements, which arise in all bodies of moving water, and are in complex proportion to the velocity of the stream and the character and condition, or, as we may say, to the accidents of the bed.

To consider these hypotheses would extend this paper beyond due limits. It is sufficient to say that the gradation of velocities in a vertical direction, so as to present a curve of velocity regularly increasing from bottom to surface, is not verified by observation. As the velocities in a vertical approach a regular curve silt conveyance ceases; and conversely, as silt movements increase the vertical curve of velocities becomes an irregularly waving or serrated line. I therefore reject the first hypothesis.

The second hypothesis, of a general upward tendency of water and immersed bodies, implies a converse proportion of

a downward movement in equal volume. Since movements in opposite directions cannot coexist in place and time, a division of the stream into ascending and descending areas is needed, or the movements must alternate at short intervals. In either case, the facts would come within the range of observation. Failure to detect the supposed movement in the general form required by the hypothesis leads to its rejection.

The third hypothesis differs from the second by a prefix only. It attributes suspension to irregularities of motion. We can conceive a stream flowing in a polished bed with a gliding, straightforward movement in every part, and such an idea underlies theoretical discussions in which fillets or layers are imagined. In real channels the roughness of bottom is sufficient to secure a thorough mingling of the waters; but the interior movements will be too feeble to sustain solid matter if the bed be smooth and the area large.

Irregular motions manifest themselves in boils, eddies and whirls. That boils bring mud as well as water to the surface is a matter of common observation. At a position where the normal quantity of sediment was, at surface 2.278, mid depth 2.427, and one foot above bottom 2.880 ounces per cubic foot, samples taken from a boil gave at surface 3.006, at 15 feet depth 3.755, and at 37 feet depth (one foot above bottom) 2.706 ounces to a cubic foot. Of course it is not certain that any except the surface specimen was taken from the heart of the boil, for the rising path must necessarily be guessed at in attempts to reach it at lower depths.

A boil originates at the bottom and may be caused by any obstruction which deflects the impinging current.

The irregularities of bottom which may deflect currents are numberless, and the deflections may be in all possible forward directions, and the impulses may have any degree of intensity within the limit of the stream's velocity. Of the whole number it is certain but a small proportion will have a direction and intensity that will bring them to the surface; but the number of boils appearing at the surface is probably in proportion to that of the unseen, so their visible increase may be taken as marking an increase of the forces and motions below.

In bringing forward this hypothesis nothing new is involved, for all more or less distinctly admit that the irregular motions share in the work. They who hold the theory of relative velocity in any form are forced to ascribe to the whirls and boils the mingling of material equal in size but differing in density, or of different sized grains of equal density at a given level; also the suspension of all matter whose density is greater than water above the axis of greatest velocity. This third hypothesis goes but one step further in ascribing to the irregular movements the whole work of suspension, upon the ground that a cause, known to exist wherever the fact to be accounted for occurs, and admitted to be efficient, must be considered the sole cause unless a coworking agency is known, or the cause is insufficient to produce the observed result. Observation readily detects whirls, boils and eddies in the act of bringing water and suspended material from the bottom to the surface, and laterally from the side to the center of the river. Observation has never detected any other cause incident to the flow of streams which produced these effects.

Once impressed by the fact that a body of moving water is an involved network of irregular movements or jets (so to speak), a person can easily understand that the power to separate solids from the bed and maintain them in suspension is dependent upon the direction and energy of the jets. The jets have their origin at irregularities of the bed, by which parts of the stream are deflected from their regular forward course. Their number and energy will ordinarily increase or diminish with the velocity of the stream. So, in a restricted sense, we may say that power of suspension varies with the velocity; but, since the relation is not direct, being through an intermediate agency, there are many exceptions to the apparently general rule that a quickened current takes up an additional load, and its converse that a check of the current will determine a deposit. If, for instance, the stream pours over a reef, with gentle backward and steep forward slope, there is in approaching the crest a quickening of velocity without increase of burden, for the crest is not worn down. In passing the crest the sensibly horizontal movement becomes a plunge, the for-

ward component of movement is materially reduced, the vertical is greatly increased, the deflected current impinges upon the bottom and does work in excavating a deep hole, expending upon that work a portion of its force. The reflected current returns toward the surface with an increased load of sediment which may be borne to a considerable distance before the subsidence of the agitations caused by the plunge will allow it to fall to the bottom. We thus see that the lessening of silt burden depends upon the manner of the slackening, rather than on the fact of slackened current. If the motion is diminished because of increased area without abrupt change of direction, deposit will ensue, but if the slackening be the effect of change in direction increased irregular motion is the first result, and with it increase of suspending power.

Erosion at the origin of a jet is the natural consequence of impact and reflection of currents if the bottom be yielding. Detached matter is kept in suspension by the interfering currents through a succession of impulses. The earlier and rising part of a jet, whether it be a mass projected by a momentary or a stream by a continuous impulse, is concentrated and energetic; later it becomes diffused and weak; therefore the resultant of impulses originating at the bottom is always upward.

We have now reached a more definite idea of the cause of suspension. It is a result of work done upon the bed and banks. The internal movements which prolong suspension are another form of work. Work may be done by a body whose motion is diminishing or increasing. It is, therefore, well to keep in mind the stricter definition of the cause of suspension, and consider it as associated with, rather than caused by velocity. Failure to observe the distinction will often lead to unsound reasoning, and to disappointment in results realized when projects are executed.

We have seen that transportation of silt (up to the point of impaired fluidity) is not at the expense of the stream's motion. The work of erosion and suspension is done by the stream, whose velocity must be diminished compared with flow under a like head in a smooth channel, but if the now yielding bed should suddenly become rigid the same, or even greater, force would be expended upon the obstructing roughness. Therefore, though suspension consumes a part of the stream's force, the velocity is not necessarily lessened beyond what it would be in the only alternative condition that can be considered, a rigid bed equally rough.

The burden of suspended matter is not the same for a given velocity when found at different places, nor at the same place at different times, because irregular movements increase or diminish under local and temporary conditions. The maximum load was found in nearly every observation several hundred feet nearer the Missouri shore than the line of greatest velocity; therefore equal velocities on either side of that line were attended by dissimilar silt burdens. It may be well to add, that in any river section the profiles of velocity and depths follow each other so closely that one may be taken as the reflection of the other; hence supposed variation of suspension with depth cannot account for the difference observed.

In attributing suspension of solid matter to irregular internal motions, advance is made from the somewhat vague idea of unsteady motion or pulsation of currents suggested by some observers to a more definite interlacing of stream lines, whose resultant is an upward thrust.

In a great river the inequalities of the bed act an important part in developing scouring power at the bed, and sustaining power throughout the extended mass of considerable depth and width. But this statement must not be understood as implying that roughness of channel would facilitate the transport of sewage matter. In the limited channel of a sewer or water conduit, the intermingling required to maintain suspension is sufficiently provided by contact with the comparatively large wet surface. Removal of deposits requires additional force.

Having now shown the volume approximately, and the immediate cause of silt movements, we are prepared to consider the consequences of variation in this cause, or in the ability of the river to carry its burden.

Clearly this variation may be due to season, comparing high and low stage, or to locality, comparing areas and form of cross section, also to the varying inclination of surface.

This division of the subject is directly practical, for a man's judgment, as to the merits of rival proposed methods of dealing with the Mississippi, will turn according as he accepts or rejects the following statements of fact, and the legitimate inference from them.

"The History of the St. Louis Bridge," by our fellow member, Prof. C. M. Woodward, records the fact that the river bed at the bridge site deepened in time of high water. The engineers, who have been engaged at Horsetail Bar, 10 miles below the bridge, have repeatedly stated that the bottom there rises during the flood and is cut out by the falling river.

These apparently conflicting statements put us upon the inquiry why opposite results should attend a high stage at the two localities?

The known variation of bed at Horsetail is ten feet, measured in the channel. That is to say, a channel 4 feet deep was found at lowest fall and winter stage. A moderate rise of 20 feet following, the river after falling 6 feet afforded a channel depth of but 8 feet, whereas 4 + 20 — 6 = 18, a difference of ten feet. The former channel was not only obliterated, but every square yard of area in a limited length of river, which was covered by water at the low stage and all the area then dry, was filled in during the rise to a height of at least 6 feet above the low-water surface of the preceding season.

The observed vertical variation of bed at the bridge site is said to have been 15 feet.

Observations made in 1878-1879 at a series of sections in front of St. Louis and below the bridge gave the following results:

Section.	Width.	Change in Area.			Change in Mean Depth.		
		Sept. 17 to Mar. 31.	Mar. 31 to May 10.	Sept. 17 to May 10.	Sept. 17 to Mar. 31.	Mar. 31 to May 10.	Sept. 17 to May 10.
	Feet.	Fill sq. ft	Fill sq. ft.	Fill sq. ft.	Ft.	Ft.	Ft.
Pine street	1,604	2,583	2,255	4,838	−1.61	−1.41	−3.02
Chestnut street	1,609	5,141	575	5,716	−3.45	−0 35	−3.80
Market "	1,650	3,306	1,091	4,397	−2.00	−0.61	−2.61
Walnut "	1,744	1,143	2,920	4,063	−0.65	−1.62	−2.27
Elm "	1,815	4,119	4,082	8,201	−2.26	−2.23	−4.49
Myrtle "	1,885	5,523	2,622	8,145	−2.91	−1.40	−3 31
Spruce "	1,942	4,520	4,546	9,066	−2.27	−2.32	−4 59
Almond "	1,982	3,695	2,907	6,602	−1.49	−1.46	−2.95
Poplar "	2,032	4,523	547	5,070	−2.17	−0.26	−2.43
Plum "	2,033	3,930	1,291	5,221	−1.69	−0.63	−2.32
Cedar "	1,958	5,792	− 327	5,465	−2.96	+0.17	−2.79
Mulberry "	1,893	1,978	5,206	7,184	−0.86	−3.06	−3.92
Pittsburgh Dike	1,547	3,180	8,268	11,448	−2.02	−5.18	−7.20
Means	1.822	3,802	2,768	6,578	−2.02	−1.56	−3.58

The stage, from September 1 to May 31, was by means of 10-day periods:

Days.	1878.				1879.				
	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Mar.	Apr.	May.
1—10	10.9′	9.6′	9.8′	9.4′	8.0′	9.3′	7.6′	12.2′	11.9′
11—20	9.7′	10.0′	.	8.2′	8 6′	8.9′	11.3′	16.4′	11.0′
21—30-1	9.2′	10.7′	.	6 3′	9.2′	7.5′	11.4′	13.1′	11.0′

The fill which occurred between September 17 and March 31 must have been made when the stage was low and the water comparatively clear. If the fill was interrupted by the rise in April, which reached an 18-foot stage, it was resumed and the apparent result is a continuous fill up to May 10. The reported scour at the

bridge by a flood involves a converse proposition of fill at some other stage, else the section would sink to bed rock and become permanent; this inference is fully supported by the behavior of the sections below the bridge, stated above.

The area of cross section at Pine street was 41,000 square feet, stage 9 feet; the area added by a 30-foot stage would be 38,000 square feet; total, 79,000 square feet. At Horsetail the river was 4,800 feet wide, and area at 9 foot stage less than 20,000 feet, there being scant 6 feet in the channel. The area which would be added by a 30-foot stage equals 103,000 square feet; total, 123,000. Comparing areas at the two localities we have for 9 foot stage at Pine street 41,000 square feet, at Horsetail 20,000; 30 foot stage at Pine street, 79,000 square feet, at Horsetail 123,000. Since mean velocities are inversely as the areas it follows that the current at low stage must be slack in front of St. Louis and swift at Horsetail; at high stage, on the other hand, it must be swift at St. Louis and comparatively slack at Horsetail; or else alternate fills and scours must occur at the two localities to equalize areas. Since the cause of suspension commonly varies with the velocity, fills and scours will occur, and a partial compensation of areas be made. Considering the material by which the compensation is effected, we see that material scoured by the flood current from the narrow section passes to the succeeding wide reach and is there deposited: at a low stage the area in the wide reach becomes deficient, velocity increases and scour ensues: the material is borne only to the succeeding and sluggish pool.

I have presented the facts in front of St. Louis and at Horsetail at large because they are, when taken together, typical of what does and must take place elsewhere wherever like contrasting conditions occur.

The movement of coarse, heavy material is now seen to be a succession of steps; out of the pool into the wide reach in time of flood, and out of the wide reach into the succeeding pool during the subsequent low stage, with a period of rest intervening for individual particles, but a continuity of movement in the aggregate.

It has been thought by some that deposit cannot occur if the velocity be greater than that usually believed to be capable of moving a given material. And, since the velocity everywhere at high stages exceeds the supposed limit required by sand, it is argued that flood time is a period of universal scour, and the deposits observed to occur must be made at the turn and during the falling stage. In answer, it may be said that the change of velocity at transition from rising to falling stage is much less than occurs from variation of section at succeeding localities at high and low stages indifferently. But it will more effectually dispose of this idea to ask where does the material eroded by a flood go to, if scour be general and several feet in depth? And, if fill be general at falling stage, where does the material come from? The alternate movement between pool and shoal alone meets this difficulty about source and storage of material.

Summing up the results of the study it appears:

1st. The cause of suspension is found in the irregularities of motion resulting from work done by the stream upon the bed and banks. These irregularities in general vary with the velocity of current and accidents of bed and bank.

2d. Variation in the power of suspension as the water passes along its course is largely due to relative values of sectional area, and these depend chiefly upon width and form of section.

3d. Scour and deposit are, under the conditions now existing in the Mississippi, local, not general occurrences, which alternate at a given locality as the river passes from one extreme of stage or volume to the other.

To enforce these conclusions I add this final fact: The volume of matter in suspension which passed the foot of Bullerton towhead between November 13, 1879, and January 3, 1880, was 702,000,000 cubic feet. The amount which passed Fulton (nine miles below) during the same time was 1,011,000,000 cubic feet; wherefore the difference, 1,011,000,000 − 702,000,000 = 309,000,000, must have been obtained by erosion of the intermediate bed and banks. Between March 6, 1880, and April 11, the volumes were: Past Bullerton, 907,000,000 cubic feet; past Fulton, 832,000,000 cubic feet, 75,000,000 cubic feet being deposited in the intermediate bed.

Bullerton is at the foot of a wide shoal reach. Fulton is at a narrow part of the river. During the first period the river rose 26.6 feet, during the second it fell 6 feet.

The results, it will be observed, are similar to those at St. Louis and Horsetail, but the measured quantities show the variation in suspension directly. The succession of contrasting local conditions is reversed, the pool following the shoal.

Variation of width and form of section have appeared to be important factors in determining the alternate movement of heavy material from pool to shoal, and from shoal to pool, as stage or volume increases and diminishes. It is also certain that an indefinite volume of heavy material is brought into the bed of the river every year by tributaries, and a like volume must be discharged at the mouth, in order that a stable or permanent condition may be maintained. Under the alternations, arising from varying width, the river, taking its whole length, is encumbered with the accumulated detritus of a number of years, all traveling. If a summation be made in cubic units of the whole mass moved in a given time, it will exceed the actual discharge of solid matter manifold, but if it be measured as mass moved over space, the summation will be equal to the movement of the incoming material from the point of entrance to the sea.

If we consider that the material passing a given point can travel but a short distance in a day at the farthest, we see that the sum total of moving material must be enormous. For instance, the summation on July 10, 1880, must have been several hundred times the quantity observed at Fulton, 3,232,209 cubic yards, or a total measured by hundreds of millions of cubic yards.

Considering the movement in time of flood we must remember that the arrest of motion due to variation of section is not of the whole mass, but of the coarser part only, and the arrest of this part need not be an absolute stoppage, in the sense that no coarse material passes the wide reach during the period of accummulation. If a column of men be marching along a street at an unequal rate, the center moving more slowly than the extremes, the advance files will widen the intervals, and the rear will at the same time close in upon the slower center. This center illustrates the wide places in time of flood. If again the front and rear slacken step and the center hastens, the massing will be in front, and intervals will increase at the center and toward the rear. The illustration now becomes parallel to motion from wide to narrow sections at low stages.

The changes from accumulation to waste (deposit to scour), therefore, do not imply a cessation of movement at any place or time, but may be accounted for by a varying rate of progress.

If the variation of width be suppressed by "regularization," one of the local complications of silt movements will be removed, the rate and volume will still vary with the motive forces. The movement of heavy bar-forming material is now continued during low stages, but in the regulated channel there will be no concentration of force to enable low stages to work. Will it be possible to secure the through conveyance of the incoming material by the flood? or must we expect the movement to be in wave masses, moved only at high stages? Experience abroad seems to indicate the latter. The problem will then be to reduce these masses to harmless proportions.

BLAST FURNACES IN GREAT BRITAIN.—The following is the number of blast furnaces at work in different parts of Great Britain at the present time: Cumberland, 42; Derbyshire, 41; Durham, 26; Gloucestershire, 2; Hampshire, 0; Leicestershire, 2. Lincolnshire, 15; Lancashire, 34; Northamptonshire, 15; Nottinghamshire, 2; Northumberland, 4; North Staffordshire, 27; South Staffordshire, 48; Somersetshire, 1; Shropshire, 9; Wiltshire, 3; Yorkshire, West Riding, 29; Yorkshire, North Riding, 90; North Wales, 6; South Wales, 65; Scotland, 107; total, 568. The total number of furnaces built September 30th, 1882, 940; total number of furnaces in blast September 30th, 1882, 568; increase in the number of furnaces built since June 30th, 1882, 4; decrease in the number of furnaces in blast since June 30th, 1882, 2; furnaces blown out since June 30th, 1882, 19.

ON THE PROPOSED FORTH BRIDGE.

Communication by Sir GEO. B. AIRY, and a Rejoinder by Mr. BENJAMIN BAKER.

An interesting account of the plan of the railway bridge for crossing the Forth at Queensferry, as designed by our distinguished engineer, Mr. Fowler, with the association of Mr. Baker, was given by Mr Baker to the British Association at their late meeting at Southampton. Supported as it was, to the advantage of those present, by the exhibition of the model of the proposed bridge, it must have given extensive information on the character of the structure. Yet it seems to me that, amidst many valuable particulars, on the strength of materials, their mode of application in this instance, and similar important subjects—it would hardly impress sufficiently, upon the minds of hearers or readers, the vastness of the scheme, the novelty of its arrange ments, and the dangers (yet untried) to which, conjecturally, it may be subject. I have thought therefore, that I might, without impropriety, offer to the editor of *Nature* some remarks on points which after careful consideration have suggested themselves to me. For some particulars I am indebted to the courtesy of Mr. Fowler himself. and I greatly value this kindness.

It is known that at Queensferry the separation of the river banks, or rather that of the piers next to the banks at the elevation required for the railway, approaches to a mile. This space is divided by three piers (for which there are excellent foundations on rock and hard clay) into four parts, but only the two middle parts concern us now. They are exactly similar, and are treated in exactly the same way; and subsequent allusions, referring ostensibly to one, are to be considered as applicable to both. Each of the three piers is an iron frame, 350 feet high, the central pier 270 feet wide (in the direction of length of the bridge), and each of the others 150 feet. These lofty frames are braced, each upper angle on one side to lower angle on the other side, with no other diagonal bracing, but with a single tie at mid-height. The lengths of the diagonal bracing are respectively about 430 and 360 feet. The water-spaces between two piers are each about 1700 feet, and the engineering question now is, how this space of 1700 feet (roughly one-third of a mile) is to be bridged for the passage of a railway.

The plan proposed is, to attach to each side of each frame (that is, to each side which will face a traveller entering upon the bridge) a framed cantilever or bracket about 675 feet long (that is, exceeding in length an English furlong by 15 feet), attached at top and bottom to the iron frame above mentioned, but having no other support in its entire length of 675 feet. To give the reader a practical idea of the length of this bracket, I remark that the length of St. Paul's Cathedral, outside to outside, is exactly 500 feet; and thus this bracket, which is to project over the water without any support whatever, is longer than the Cathedral by 175 feet. This in itself is enough to excite some fear, supposing the bracket to support merely its own weight. But further, the bracket bears also the very considerable weight of the roadway and rails. It is also heavily loaded on its point. The two opposing brackets from the two iron frames cover 1350 feet, but the whole space to be covered is 1700 feet, leaving 350 feet yet to be supplied for the support of the railway. To furnish this, a lattice-girder carrying a railway is provided, rather more than 350 feet long, whose extremities rest upon the tips of the two brackets.

This statement is enough, I think, to justify great alarm. No specimen, I believe, exists of any cantilever protruding to a length comparable, even in a low degree, to the enormous brackets proposed here. The only structures of this clsss, in ordinary mechanics, known to me, are the swing-bridges for crossing dock-entrances, and the like, and these are absolutely petty in the present comparison.

I now advert to the weights of the principal portions of the bridge, and the strains which they will create. I understand that the weight of the two parallel

braced sides of one bracket is about 3360 tons, to which is to be added the weight of roadway and rails for 675 feet, on which I have no information. I proceed to inquire what strains, in the nature of horizontal pull at the top of the pier and horizontal push at the bottom of the pier, will be caused by this weight. If the weight were evenly dispersed over the triangular bracket, its centre of gravity would be distant from the pier by one-third of the distance of the point from the pier. But as no vertical bar near the pier is included in the weight above, I must take a larger factor, say $\frac{2}{3}$. The vertical weight being 3360 tons, acting at a distance from the pier of $\frac{2}{3}$ x 675 feet, and the separation of the points of connection with the pier being 350 feet, it is easily seen that the horizontal pull at the top and push at the bottom are each about 2600 tons. The inclined tension along the great upper bar of the cantilever and the inclined thrust along the great lower bar of the cantilever are therefore each about 2670 tons. The extremities of the great upper bar and the great lower bar being connected at the point of the bracket, and (for a moment) no other weight being supposed to act, there is no tension or thrust at that point, and therefore the tension and the thrust increase gradually, according to the attachment of their loads, from nothing at the point of the bracket to 2670 tons at connection with the pier.

But the point of the bracket is permanently loaded with half the weight of the intermediate 350-feet railway, or 363 tons, and occasionally loaded with the whole weight of a railway train, say for a passenger train 150 tons (a mineral train would be heavier). The vertical weight of 513 tons thus introduced would be met by a tension of 1004 tons through the whole length of the great upper bar, and a thrust of 1004 tons through the whole length of the great lower bar. Thus we have—

For the great upper bar, a tenison increasing from 1004 tons near its point, to 3674 tons near the pier.

For the great lower bar, a thrust increasing from 1004 tons near its point, to 3674 tons near the pier.

The second of these statements particularly requires attention.

Mechanical students and professional engineers are accustomed to estimate by numerical measure the magnitude of a horizontal or nearly a horizontal thrust but persons in ordinary life scarcely attach a clear meaning to such a phrase. I am therefore compelled to make a somewhat violent explanatory supposition, with the hope that it may convey a practical impression as to the meaning of the statements just given.

The great lower bar is in fact a nearly flat frame, braced from side to side, about 120 feet wide at the bottom, and about 40 feet wide at the top, and 690 feet long. Suppose this structure to be planted vertically, say in St. Paul's Churchyard, without any bars, chains, or any thing else, below its vertex, to prevent motion edgewise, but with bracing (which, under ordinary circumstances, would suffice, but which will be the subject of further remark) to prevent its moving flatwise. Its top would be 310 feet higher than the top of the cross of St. Paul's Cathedral. Suppose a weight of 1000 tons to be placed on its very top, and additional weights (if necessary) to be placed at its sides, till the whole weight pressing the ground is 3600 tons. In this state its condition is exactly that of the great lower bar, as regards the crushing and distorting tendency of the weights (although the upper weight itself ought to be considered as partially protected from lateral movement by the great upper bar). With this enormous load at this stupendous height, would the citizens of London in the Churchyard below feel themselves in perfect security? I think not; and I claim the same privilege of entertaining the sense of insecurity of the proposed Forth bridge.

The danger arising from the endwise action of so large a force on so long a bar or frame, is produced by the curvature technically called "buckling," and there appears to be fear of its occurrence in various parts of the bracket, and in some parts sequentially, that is to say, that a buckling of a minor order might lead to a buckling of a more important order. Thus, proceeding from the pier, the first support of the great lower bar is by a suspension rod from the great upper bar, to which, as regards merely the suspension-rod, there can be no objection. But the upper attachment of this suspension-rod is supported by a

thrust-rod about 340 feet long. Can this rod be considered safe against buckling? In the total absence of experiment or explanation, I may be permitted to express a doubt of safety. And if that rod fail, the corresponding part of the great lower bar will sink, it will buckle under its enormous end-thrust, and the bridge will be ruined. The second support of the great lower bar depends, in like manner, on a thrust rod whose length is 240 feet; considerations of the same kind apply to it, though probably in a minor degree.

Experienced engineers must have known instances in which buildings have fallen from want of consideration of buckling. The following occurred within my knowledge. When the Brunswick Theatre was built, the construction of its trussed iron roof was greatly extolled, and Mr. Whewell and myself, then residing at Cambridge, and proposing to visit London about the same time, had arranged to inspect the truss. But before we reached London it was ruined. There was no adequete bracing of the principal rafters in the plane of the roof; the suspension of a very slight weight on the great tie caused the rafters to buckle sideways, and the roof fell, destroying the building.

I am not aware whether a theory of buckling finds place in any of the books which treat of engineering in somewhat mathematical form. But there ought to be such. It can be formed with no difficulty and little trouble, giving such a form of result, that all that will be required in any case, to determine the end-pressure which can safely be applied to the end of a bar, will be expressed in terms of the length of a bar, and the curvature caused by a transversal strain (determined by simple experiment). This theorem ought to applied in every instance.

I need scarcely to remark that every construction is liable to chance-errors of unforeseen character, and I think that the proposed construction, which depends for its safety entirely on the maintenance of the thrust-principle in perfection, is more liable than any other to danger from these causes. A rivet-head may slip, or a screw may strip, and all may be imperilled. Robert Stephenson, when the Menai Bridge, used every caution that an active mind could invent: in particular he provided that the masonry for final support of the tubes should be raised as quickly as possible to take the bearing of the tubes at every moment. Yet an accident, though a small one, did happen. The ends of the tubes were raised by the power of hydraulic presses; the cylinder of one of these presses burst, and the end of the tube fell three or four inches. This minute fall, in the judgment of the attendant engineers, gave a strain to the tube such as it never sustained before or since. (This accident came first to my knowledge in a singular way. With the assistance of my friends, Captain Tupman, R. M. A., and James Carpenter, Esq., and before having heard of the accident, I made experiments on the state of permanent magnetism of the great iron tubes. One of these showed an anomaly, somewhat similar to that of iron heavily struck. On my mentioning this to Mr. Edwin Clark and others, the phenomenon was at once referred to the accidental shock which I have described.)

Much has been said on the action of the wind, and on the difference of that action upon a suspended bridge, and upon a girder bridge. In regard (first) to the amount of pressure, I refer to a former letter of mine, correctly cited in the evidence before the Committee on the fall of the Tay Bridge, in which I state that the maximum pressure may be more than 40 lbs. on the square foot (I should say more than 50 lbs. for Scotland), but that this action is so limited, both in time and in local extent [and is, I add, so continually varying in direction], that the average of direct pressure probably would not exceed 10 lbs. on the square foot. In regard (secondly) to the difference of wind-action in the two systems of construction;—the immediate effect of the wind appears to me to be a shock, of limited extent, which is much less likely to be injurious on a comparatively flexible frame suspended from above, than on a jointed frame where every joint must be tight, and where ruin will follow disturbance. In the proposed Forth Bridge, however, there is a risk of danger of the most serious kind, which may perhaps surpass all the other dangers. It arises from the horizontal action of the wind on the great pro-

jecting brackets, and its tendency to wrench them laterally from their attachments. The ruinous force depends, not simply on the magnitude of the wind's pressure, but also on its leverage; as measured by the proportion of the height of the Tay Bridge or the length of the bracket of the Forth Bridge, to the separation (in each case) of their horizontal attachments to the solid piers. The leverage is considerably greater in the instance of the proposed Forth Bridge than it was in that of the unfortunate Tay Bridge, and we may reasonably expect the destruction of the Forth Brdge in a lighter gale than that which destroyed the Tay Bridge.

I may now collect the heads of my remarks on the proposed Forth Bridge:

I. The proposed construction is, as applied to railway bridges, entirely novel.

II. The magnitude of its parts is enormous.

III. There has been no succession of instances of the construction, with the rising degrees of magnitude, which might furnish experimental knowledge of some of the risks of construction.

IV. The safety of the bridge depends entirely on a system of end-thrusts upon very long rods; a system which appears generally objectionable, but particularly so when the length of the rods is very great.

V. No reference is made to theory applied to the buckling of rods under end thrusts.

VI. The liability to ruinous disturbance by the lateral power of the wind acting with the leverage of the long brackets appears to be alarmingly great.

My own impression is, that the proposed construction is not a safe one, and I should be happy to hear that it is withdrawn.

I refer unhesitatingly to "the Suspension Bridge" as the construction which I should recommend. On this system generally I remark: (1) that I am incredulous as to the oscillation of 8 feet in extent, or any sensible part of it; (2) that if the railway is slightly arched upwards to the degree corresponding to depression caused by an average train, such a train will run on a horizontal plain; (3) that a stiffening lattice may be used with very good effect against vertical oscillations from all causes.

The considerable height of the piers, and the great length of the suspension-chains, are matters to be viewed carefully.

To reduce them as far as possible, I would suggest for examination the following proposals:

1. Suppose the stone or iron piers to be much lower than the plans hitherto proposed, and suppose that the top of a pier carries a bracket on each side, so that the great suspending chain passes over the points of the brackets, and its suspending action begins at those points. The bracket frame may be horizontal where it passes the top of the pier; or it may be raised in a horn on each side, and thus adapted to a smaller height of pier. By this construction, with brackets 150 feet long (a trifle compared with those of the proposed cantilevers), the piers may without difficulty be shortened 200 feet, and the acting-length of suspending chain may be reduced 150 feet at each end, or 300 feet over each water-channel. This would leave much liberty in regard to the curvature of the chain.

2. It is very desirable, if possible, to reduce the specific weight of the chain per yard, corresponding to a specified suspension strain. This has been attempted on the Continent by the use of wire, and it has been highly praised for its combination of lightness and strength. The longest carriage-bridge that I have passed (that of Freyburg, 890 feet span) is a wire bridge. I have also crossed the Rhone at Montelimart by wire arches of considerable span. I know not whether this construction has been tried in England.

G. B. Airy.

I have read Sir George Airy's criticism of the design for the proposed Forth Bridge with interest. So far as engineers are concerned the letter calls for no reply; but as others pardonably ignorant of the present state of engineering science may feel the same difficulties as Sir George Airy, I propose with your permission to offer a few explanations.

Sir G. Airy summarizes his remarks under six heads, but I think two would have sufficed, viz.: that the bridge was too big to please Sir George, and that

the engineers were presumably incompetent. As to size, for example, Sir George considers the fact of the cantilever being "longer than the Cathedral by 175 feet is in itself enough to excite some fear," and even to "justify great alarm." But when I look for some justification for this bold statement I find that Sir George does not advance any reason whatever, nor make use in any way of his high mathematical attainments, but simply shifts the responsibility for this alarm on to the shoulders of the "citizens of London," asking "would they feel themselves in perfect security? I think not; and I claim the same privilege of entertaining the sense of insecurity for the proposed Forth Bridge."

If Sir George had alleged that the stresses on the cantilever could not be calculated, or that the strength of the steel ties and struts could not be predicted, or that the cantilever could not be erected, I might have replied by publishing diagrams of stresses, results of experiments, and the names of the firms who have tendered for the work. I cannot, however, answer an argument based upon the supposed fears of the "citizens of London."

To prove that Sir George's criticisms imply a charge of incompetency on the part of the engineers, I need only point out that in one sentence he remarks that "experienced engineers must have known instances in which buildings have failed from want of consideration of buckling," and in another, that "there appears to be a fear of its occurrence in various parts of the bracket," when "the bridge will be ruined." Sir George's conclusions on this head are, however, as he fairly enough states, "made in the total absence of experiment or explanation," and in ignorance whether "a theory of buckling finds place in any of the books which treat of engineering." To assume, however, that an engineer is similarly ignorant, clearly amounts to a grave charge of incompetency. Again how incompetent must the engineer be who required to be informed that the "horizontal action of the wind on the great projecting brackets depends not simply on the wind's pressure, but also on its leverage," or who neglected to provide for the consequent stresses. Yet Sir George does not hesitate to say in reference to this, that "in the proposed Forth bridge there is a risk of danger of the most serious kind, which may perhaps surpass all other dangers."

As Sir George in the whole of his letter does not produce a single figure or fact in support of his very serious charges, I must, in justice to Mr. Fowler and myself, explain that it was from no want of data. At Sir George's request he was furnished with every necessary detail for ascertaining the maximum stress on each member, and the factor of safety. I stated in the paper referred to by Sir George at the commencement of his letter, that under the combined action of an impossible rolling load of 3400 tons upon one span, and a hurricane of 56 lbs. per square foot, the maximum stress upon the steel would in no case exceed 7½ tons per square inch. Any useful criticism must be directed to prove that such load is not enough or that such stress is too great. Nothing can be decided by appeals to the citizens of London.

Sir George's remarks about what he terms "buckling," and the "total absence of experiment," I can hardly reconcile with his having read my paper, because I have there devoted six pages to the question of long struts, and have given the results of the most recent experiments on flexure by myself and others. When he asks whether a tubular strut 340 feet long would be safe against buckling, he has evidently overlooked the twenty years' existence of the Saltash bridge, which has a tubular arched iron strut 455 feet long, subject to higher stresses than are any of the steel struts in the proposed bridge. Reference is made to the fall of the roof of the Brunswick Theatre, which is attributed to buckling. This accident occurred about fifty-four years ago, and consequently considerably before my time; nevertheless I have heard of it often; and if I am not mistaken, the verdict of the jury was to the effect that the fall of the roof was due to a carpenter's shop weighing about twenty-five tons having been built on the tie-rod, which sagged under the weight, and so pulled the feet of the principals off the wall. However that may be matters little, as engineers are in possession of more recent and trustworthy data than the personal reminiscences of Sir

George Airy. American bridges invariably have long struts, and consequently there is no lack of practical experience on the subject.

The late Astronomer Royal thinks that "the proposed construction is not a safe one," and hopes to see it withdrawn. When he wrote his letter it probably did not occur to him that rival railway companies might be only too glad to seize hold of anything which might prejudice the Forth bridge project and alarm the contractors who were preparing their tenders for the work. I do not complain of Sir George's action, as it involves a matter of taste of which he is sole judge. I would only mention that when he penned the above sentence he had been furnished by the engineers with the parliamentary evidence and other documents necessary to inform him of the following facts:—(1) That a wind pressure of 448 lbs. per square foot upon the front surface would, as stated in my paper on the Forth Bridge, be "required to upset the bridge, and under this ideal pressure, though the wind bracing would, it is true, be on the point of failing, none of the great tubes or tension members of the main girders would even be permanently deformed." (2) From the evidence given before the Tay Bridge Commissioners, Sir George, being a witness, would know that, even supposing the workmanship had been good, a wind pressure of about one-tenth of the preceeding would have sufficed to destroy the Tay Bridge. (3) He would also remember, no doubt, his own report of 1873, wherein he says that "the greatest wind pressure to which a plain surface like that of the Forth Bridge will be subjected in its whole extent is 10 lbs. per square foot." (4) The Parliamentary evidence would have informed him that the proposed design was the outcome of many months consideration by the engineers-in-chief of the companies interested, representing a joint capital of 225 millions sterling, and that it was referred to a Special Committee of the House of Commons and to a Special Committee of the Board of Trade inspecting officers for examination and report, and that the reports of engineers and committees were alike unanimous in testifying to the exceptional strength and stability of the proposed bridge. As a sample of foreign opinion, I would quote that of Mr. Clarke, the eminent American engineer and contractor, who has built more big bridges himself than are to be found in the whole of this country, and who has just completed a viaduct 301 feet in height, by far the tallest in the world. Referring to the proposed bridge, he writes: "If my opinion is of any value I wish to say that a more thoroughly practical and well considered design I have ever seen" I need hardly say that the opinion of such a man has far more weight than that of an army of amateurs.

Sir George Airy refers "unhesitatingly to the suspension bridge" as the construction which he should recommend. He has clearly learnt nothing on that head during the past ten years. In a report on the late Sir Thomas Bouch's design for the Forth Bridge on the suspension principle, dated April 9, 1873, he says: "I have no doubt of the perfect success of this bridge, and I should be proud to have my name associated with it." Chiefly on this recommendation, and in spite of numerous warnings from practical men, the bridge was commenced, but it had to be abandoned after spending many thousands, because having reference to the fate of the Tay Bridge, it was pronounced by the Board of Trade and every engineer of experience at home and abroad to be totally unfit to carry railway trains in safety across the Forth.

Sir George Airy stands alone in his advocacy of a suspension bridge for high speed traffic, and in his views as to the force and action of the wind on such a structure. That being so I may be permitted to say that I should have felt no little misgiving if he had approved of the substituted girder bridge, because it has been the aim of Mr. Fowler and myself to design a structure of exceptional strength and rigidity, differing in every essential respect from that with which Sir George evidently would still be proud to have his name associated. B. Baker.

During the half-year ending 30th June last the engines of the London, Chatham and Dover Railway made 1,627,283 train miles, and the total cost for locomotive power was £68,053.

ON THE DRAUGHT OF CHIMNEYS.

From "The Builder."

THE attention of the Mathematical and Physical Section of the British Association was, on the 30th of August, called by their president, Lord Rayleigh, to the very humble practical inquiry as to the effect of wind on the draught of chimneys. It would be difficult more aptly to illustrate the true connection between the pursuit of pure science, and the most everyday annoyances of domestic life (with a view to the removal of the latter), than is afforded by this announcement. As a prime feature in the architecture of dwelling houses the chimney demands a chapter to itself. And it is well that the basis of this chapter should be mathematical. With regard to draught, we have more than once had occasion to remark that it is a function of the height of the chimney tubes. We do not, of course, apply the remark without modification to the details of the ill-constructed chimneys. Many chimneys smoke from positive ill-construction, having a narrow part, an ugly bend, or a downward portion in their conduit, which the normal draught is not strong enough to overcome. Others smoke because they form the shortest channel by which air can enter the house to supply the draught of the loftier chimneys. Where there are two chimneys in one room, and the doors and windows are closed, lighting a fire in one chimney will ordinarily cause down-draught in the other, as the readiest mode of maintaining the atmospheric equilibrium. In most cases, however, the addition of a yard or so to the height of the chimney stack, supposing that to be already above the highest part of the interior of the house which it ventilates, will prove more useful than the addition of any of those curiously contorted and wholly non-pneumatic contrivances, the endless variety of which tortures the eye of the mechanically-minded man as he whirls along the suburban railways.

Apart from the questions of normal draught, and of internal regularity of construction, is the effect of wind on the draught of chimneys. This varies very much with the locality of the house. It may be said to be primarily a question of sight. Neither is it a question always to be settled, *a priori*, by the architect. The action of wind, although, of course, always determined by physical laws, is so subtle, and so delicately affected by slight causes, that there are cases in which it is all but impossible to foretell it. What is more common than to have a chimney that answers perfectly well, except when the wind is in one particular point of the compass? And why does it then smoke? To be able to reply to this question is to be able to cure the defect; but how often is the difficulty regarded as insuperable? It may be useful to cite an instance of the deflection of wind in a manner that it is difficult to explain, and that could certainly never have been anticipated. On the lovely plain of Sorrento, sloping towards the Bay of Naples, in the midst of its own gardens and orange groves, stands a stately palace, which, in the days when the name of Nelson was a terror on the seas, was a favorite haunt of the Queen of Naples, and was not unfamiliar to our great sea captain. There are no rocks, lofty buildings, or overhanging trees in the immediate vicinity, and nothing that is apparently likely to interfere with the free movement of the wind, from whatever quarter it may blow. The aspect of the front of the palace is northerly, looking over the Bay towards Naples, and the hills behind it. On a few occasions—some three or four times, perhaps, in the year—a strong east wind sweeps across the peninsula, and is clearly indicated as to its course by the straight and level stream of smoke that it drives from the cone of Vesuvius across the Bay. But on these occasions, the current is so deflected by the northern shores of the Bay, fully twenty miles distant, as to drive on the front of the palace; that is to say, in a direction pointing nearly due north with such force that it becomes necessary to close the *persiani*, or outer window-shutters, in order to preserve the great drawing-

room windows from being blown bodily in. Full familiarity with the spot does not afford any very satisfactory explanation of the phenomenon, of which, however, we can speak from repeated personal observation.

This, perhaps, may be regarded as an extreme case. Nevertheless, it is typical of not a few instances in which currents may be detected after a house is built, the existence and effect of which the architect cannot be blamed for not having foreseen, but the influence of which on the draught of his chimneys will be, for so many days in the year, at once irresistible and intolerable. There is now absolutely nothing for it, in such a case, but to put out the fires. We do not say that a cowl may not be of use, but, on the other hand, we cannot speak from any experience of the action of a cowl under such conditions.

Here, then, comes in the scientific part of the question as attacked by Lord Rayleigh. What is the effect of a cross current of wind on the draught of a chimney? As the direction of the wind is more and more downward, his lordship replies, the up-draught of the chimney diminishes, but does not turn backwards, that is to say, become down-draught until the inclination amounts to about 30°. This is an important point at which to arrive. It is, however, obviously necessary to ascertain the precise features of the cases where the experiments were made, before formulating any absolute rule on the subject. The height of the top of the chimney, both above the sea level and above the basement of the house, is one fact needful to determine. The force of the wind is another. Thus, a wind of the velocity of ten or twenty miles an hour blowing downwards at a measured angle would produce a very different effect on the draught of a chimney 40 ft. high from that which it would have on the draught of a chimney 100 ft. high. If our recently published table on wind pressure (*ante*, p 200) be referred to, it will be seen how important are these data for the solution of the problem.

Again, the maximum up-draught, we are told, is not with a direction of wind vertically upward, but with one making an angle of about 30° with the vertical. This is a result which it is without our experience either to confirm or to question. But it is clear that the cases must be extremely rare, if ever they occur, in which the top of a chimney would be exposed to a wind blowing upwards at a more acute angle than 30°. The determination of the same angle as the limit of downward and of upward efficiency is as elegant as it is unexpected.

A yet more practical part of the subject is the remark of Lord Rayleigh, that a "chimney with a T-piece at the top never produced an unfavorable effect on the up-draught, and only in one case failed to produce a favorable one." Professor de Chaumont thought vertical ends would increase resistance to the up-draught. So do ninety-nine people out of a hundred. We have no hesitation in saying that Lord Rayleigh is right, and that general opinion is wrong, provided, that is to say, that the protection of the chimney-top be rightly designed. And we say this absolutely and unhesitatingly, because our judgment is based on the two independent and concurrent sources of evidence afforded by observation and by direct experiment. In Southern Italy chimneys, no doubt, are comparatively few, but they exist in many buildings, and no Italian architect would dream of leaving a vertical flue or pipe open at the top to receive the tremendous downpour of rain which occasionally occurs in Italy with almost tropical violence. The aperture of the chimney is always roofed over, the smoke escaping by side apertures. The practice of the master builders of the world is thus in accordance with the theory of the English scientific noble in this respect.

Experience, we have said, confirms the remark. And here, if we seem for a moment to depart from an admirable rule—that of not mentioning in a scientific paper anything that may look like an advertisement—we must explain that such appearance is deceptive. We have to refer to an admirable invention, which was made and sold some forty years ago under the name of Day's Patent Wind Guard. As to the validity of the patent it is not needful now to inquire, as in any case it must have long since expired. But what we lament is, that, so far as we can discover, this admirable invention has been entirely forgotten. In recent inquries which we have instituted in

order to obtain one of these wind-guards for our own use, we have been unable to discover that they are now made, although figured in some old trade-books, It may, therefore, be of much use to give such particulars as will guide any one in constructing an appliance of the kind.

Let us take, for example, the case of a chimney two bricks and a half, or 22½ inches square, with a flue of 9 inches square. A frame is to be constructed which would lie upon this hollow square, and allow bricks to be built on it to the height requisite to hold it steady. From this frame the guard itself rises as an octagonal shaft. Four pieces of sheet iron, or of slate, each 9 inches wide and 18 inches high, are fixed on the lower frame, 16 inches apart, so as to form four sides of an octagon, and the other four sides are formed of four similar plates, set at angles of 45° with the former, but so much within them as to allow 1½ in. to 2 in. clearance between the edges. These inner plates will just touch the outer angle of the 9 in. flue. A top of iron or of stone, holds the upright plates together, and both top-plate and base-plate may be finished with a simple moulding. Sheet iron is perhaps the best material, but it is obvious that the contrivance may be made also in slate.

As to the perfect security which this guard effects from wind, we can speak not only from the testimony of those who have used the plan, but from careful experiment. With models of the wind-guard, made on the scale of 1 in. to a foot, set on small tubes of proportionate size, representing chimneys, which were fed with smoke from the combustion of paper, we have experimented by means of powerful jets of wind driven in various lines of force; vertically downwards on the top of the guard, horizontally at right angles to either side or angle, or in any other direction. In no case have we found it possible to produce down-draught in the chimney by force of wind. On the contrary, the greater the force the better the draught of the chimney.

The *rationale* of this action may be illustrated by that of a much later invention, now in common use on our railways—the steam injector. Let those of our readers who desire to master the subject, and have no more ready mode of doing so, refer to the diagram and description of this appliance which is given by Sir F. J. Bramwell, on p. 335 of a work recently published by Messrs. Longmans, under the title, "Railways and Locomotives." It will there be seen that "steam, escaping at high velocity through a circular orifice, induces a current in a body of water in connection with that orifice, and carries it forward at the less velocity corresponding to the increased weights of the combined stream of water and steam, but still at a high velocity." This stream passes into an expanding channel, where its velocity is further reduced, and so into the boiler, which is thus fed with water.

In the case of the wind guard, the wind striking on the outer plate causes a current of proportional velocity between the edges of the outer and inner plates, and thus creates an induced current, which comes in aid of the draught of the chimney. The difficulty to be overcome is far less than in the case of the two fluids of unequal specific gravity. But the effect is so much the more powerful. We are familiar with the contrivance mentioned by Professor de Chaumont, which has been used for the protection of lamps from the dash and spray of the sea, as we believe, with admirable results. But it is a much more complicated, and therefore more costly contrivance than the very simple one which we have endeavored to rescue from oblivion. And we think that it is purely protective in its action, while the construction that we describe is not only protective, but absolutely aids the up draught; and, moreover, the cone is more likely to become choked with soot. The brush of the sweep can easily ascend into the interior of the wind-guard, in case of any soot lodging there.

We have little doubt that any enterprising manufacturer who will discover one of these old wind-guards (they were made and sold somewhere in Pimlico, in 1842), make some to pattern, and introduce them to the trade, will at the same time do a good stroke of business, and confer a very great benefit both on the architect who has to build in exposed and gusty situations, and on many a householder who now knows too well, and that to his cost, when the

wind is in the east—or in that quarter, wherever it may be, that is hostile to the good behavior of one or more of his chimneys.

We are not acquainted with any mathematical formulæ intended for the guidance of the architect as to the height or draught of chimneys. It may, therefore, be of some service to provide a simple table for the use of the architect and of the builder, stating, in the first instance, the principles on which it is calculated.

The only cause of that upward movement which we call the draught of a chimney is the difference between the weight of the column of air and vapor in the shaft, and that of a column of equal height of the exterior atmosphere. This difference of weight must be enough to overcome the friction within the chimney shaft and any resistance to free discharge at the top. It is obvious that the ascending volume consists of matter which, at equal temperatures, is heavier than atmospheric air, as it contains soot, or finely comminuted carbon, carbonic acid, sulphur, and other matters. naturally heavier than air. Calling attention to the existence of these two sources of retardation, viz., friction and the loading of the column, it will be safe to neglect them in tabulation, as it is very difficult to determine the absolute heat of the ascending column, and a few degrees more or less will amount to quite as much as the elements thus neglected. With this explanation, the formula that regulates draught may be thus stated:

$$D=(a.t.-a.t'.)h.$$

where D=the draught, $a.t.$ the weight of a cubic foot of atmospheric air at the external temperature, and $a.t'.$ the weight of a cubic foot of atmospheric air at the mean temperature within the shaft, and h the height of the chimney. It is obvious from a glance at this formula of how much importance the addition of a very few feet to the height of a chimney must be.

To increase the draught, we must increase either h, the height of the chimney, or t', the temperature of its contents. This explains at once the efficacy of the best appliances for increasing draught—such as the bi-valvular registers, in this country, and the iron blind that is drawn down within the chimney aperture in the wood-burning fireplaces of France and of Belgium. The larger the aperture below—the greater the quantity of cool air that enters the chimney—the lower is t', and the less the draught. The more the air that enters below is compelled to pass through in close proximity with the burning fuel, the higher does t' become, and the more powerful the draught. This is the simple key to the whole theory and practice of draught produced by heat. Of course, in different conditions of the barometric column equal amounts of heat will have somewhat different effects. Hence we can see in a moment why our fires should "draw" so much better in clear cold weather than when the air is warm, or loaded with damp. The weight of a cubic foot of air at 32° Fahr. is 0.080728 lbs. avoirdupois.

If the top of a chimney-flue were closed by a flap, or hinged valve (the weight of which we take as counterbalanced), the effect of the draught, or rather of the downward pressure of the external atmosphere which causes the upward draught, would be to lift the valve in proportion to the force of the current. If at the same time a strong wind be blowing over the top of the chimney (in the direction of the hinged end of the flap), its effect would be to force down and close the valve. The stronger current of the two will obtain the mastery, to the degree of its excess of pressure. This is also the effect, it will be seen, of cross current where there is no valve, but where the two currents more or less interfere with the movement of one another in the way shown by the action of the valve.

It is possible wholly to eliminate the ill effects of this cross current, and even to utilize it in aid of draught, by any contrivance that causes the external blast to produce an induced current from within the chimney; and this, it is easy to see, is the effect of the wind-guard.

In calculating the following table we purposely use round and approximate figures, as quite sufficiently accurate for practical purposes, and as more readily intelligible to many readers than more complicated mathematical expressions. We have also used the pressures proportionate to velocity as allowed by Smeaton. Now, assuming, for conveni-

ence of calculation, an increase of temperature of 108° Fahrenheit (that is, from 62° to 170°), as imparted by a fire to the ascending column of air that passes over it up the chimney, we shall have an upward pressure of a fraction over 0.04 lb. for every foot of ascent in the shaft. This is represented in the following table of comparative heights of chimney, pressures, and velocities of current, calculated for that rise of temperature.

Table for Calculation of Chimney Draught:

Height of Chimney in feet.	Pressure due to 116° Fahr. in pounds per foot.	Velocity of draught in miles per hr.
30	1.2	16
40	1.6	18
50	2.0	20
60	2.4	22
78	2.8	24
80	3.2	26

In rising from 15 feet to 50 feet there is an increase in the local velocity of the same wind from 60 to 70.8 miles per hour. In raising a chimney from 15 feet to 50 feet we should obtain an increase of draught pressure of from 0.6 lbs. to 2.0 lbs. per foot, as against an increase of wind pressure, in a great storm, of from 20 lbs. to 27 lbs. per foot. The gain is more than threefold—the loss less than 33 per cent. We mention this, not as a difficulty, but to show that proper consideration must be given to the whole subject.

It is, of course, apparent that if we have an ordinary fireplace, with a chimney 50 feet high, and with that proportion of fuel and of draught which gives a current of twenty miles an hour to the ascending column of smoke, the aperture at the top of the chimney would be closed by a cross-current blowing at the rate of seventy miles an hour, almost as efficiently as by a valve weighted to 27 lbs. per foot. The fire, in fact, would have to be extinguished. But the pressure of 27 lbs. per foot, or 0.19 lb. per inch, is very trifling in comparison to the pressure of steam in a boiler; and as the action of the injector by induced current is an ascertained fact, it is plain that it is within the province of the engineer to make the hurricane, blow it ever so wildly, produce an induced current through a chimney, so that the draught shall in no way be reversed by storms. As to that, there can be no doubt, although this may be the first time that the statement has been definitely made. We have given what our own experience has led us to believe to be a simple and effective means of producing this induced current. If any of our readers can recommend a better plan, we shall be happy to make it known.

We had put on paper some remarks on one or two other matters discussed at the British Association which appeared to us of striking industrial importance; but the novelty of a scientific investigation of a subject which most men are accustomed to regard with a hopeless shrug of the shoulder, as well as the importance of a thorough appreciation of a matter which comes so directly home to every householder and to every house-builder, is such that we have thought it well to devote to this question of draught all the space at present available. We think that it cannot be too strongly insisted on that when, first, the subject of up-draught, and, secondly, that of wind-protection — or rather wind-induced draught—are seen to be thoroughly within the control of science, it will be felt to be unpardonable to be condemned to the great nuisance of a smoky chimney.

P. Weiskopf has given the following formulæ for the frit or mass used in Bohemia for making imitations of some of the precious stones: Imitation agates—10 kilos. quartz, 17 kilos. red lead, 3.2 kilos. potash, 2.2 kilos. borax, and 0.1 kilo. arsenic. The quantity of chloride of gold added is equal to that obtained from 0.4 of a ducat. Agate glass—10 parts of broken glass are melted, and to it are added 0.15 part suboxide of copper, the same quantity of the oxides of cromium and of manganese, 0.02 part each of oxide of cobalt and nitrate of silver, 0.01 oxide of uranium, 0.4 red argols, 0.3 part bone meal. Each oxide is added alone and at intervals of ten minutes. After heating the mixture for an hour, 0.3 or 0.4 part of fine soot is put in. Red marble—80 parts of sand, 40 of potash, 10 of lime, 2 of table salt, 1 of saltpetre, and 0.1 of arsenic. The mixture is melted and then 25 parts of suboxide of copper and 1 part of saltpetre mixed in.

THE CONSTRUCTION OF ELECTRO-MAGNETS.

By TH. DU MONCEL.

Translated from the French for VAN NOSTRAND'S ENGINEERING MAGAZINE.

I.

THE complaint is general and not without reason, that the subject of electro-magnetism is so obscurely treated by scientists, and the conclusions reached by them are of so little practical value, that the inventor or constructor can derive no profit from them. It is certain that the mathematical physicists regard such problems of too high an order to permit themselves to be diverted by a consideration of the practical applications, and it may be said furthermore that the theories of magnetism are not as yet easily expressed in mathematical symbols, and many of the so-called laws are still matters of controversy with the more conservative.

For ourselves, having made many experiments, we are less skeptical, and although we have not been able to verify rigorously the laws as propounded by Lenz, Jacobi, Dub and Müller, we have obtained results so nearly approaching to verification, that we admit them as guides in the construction of electro-magnets. Ohm's Law relating to electric currents is on the same footing, for it is difficult to take cognizance in the formula of a multitude of secondary reactions which interfere more or less with the calculated results. But these laws are faithful guides which enable us to place ourselves in conditions favorable for success, and that is the essential point.

In order to render such a scientific fact practically useful, it is necessary to be unencumbered with the hypotheses of the higher physics and those terms which but few electricians understand, and to start with experiments made under ordinary conditions. It is certain that if, to appreciate a magnetic force it is necessary to begin with the oscillations of a magnetic needle, or with the currents induced by such force, to arrive at the necessary formulas, the practical constructor can say that though such formulas afford no mental conception of magnetic force, he knows that they relate to a definite weight attracted or supported, and that the real object being to attain the greatest force under given conditions, other considerations are of little importance. It is under such conditions that it is necessary to approach the problem in order to arrive at practical results.

Now I have always admitted that the known laws of electro-magnetism were sufficient for the purposes of the artisan, and upon this idea I have based the different papers published on the best conditions of construction of electro-magnets. Wishing to be sure of my deductions, I have made many experiments in order to verify my formulas, and it was only after the most careful experimental research the writer established the formulas given.

I.—FORMULAS OF ELECTRO-MAGNETISM.

In order to establish my formulas, I start from the elements entering into the construction of the electro-magnet; knowing the dimensions of the core, the size of the wire of the helix, its length; the number of spiral turns in this helix, and its thickness.

Let a = the thickness of the helix.

b = the total length of the two spools or the two branches of the electro-magnet.

c = the inside diameter of the helix, which is presumably the same as the core or magnet.

g = the diameter of the wire of the helix, including its insulating envelope.

A = the attractive force of the magnetic system.

E = the electromotive force of the pile employed.

F = the force of the electro-magnet or its *magnetic moment.*

H = the length of the helix.

I=the intensity of the current in the circuit.
t=the number of turns in the spiral.
R=the resistance of the circuit, including the battery.

It is easily seen that we can represent the number of spiral turns of each layer by $\frac{b}{g}$, and as there are as many layers of wire as g is contained times in the thickness a, we shall have for the total number of turns t

$$(1) \qquad t=\frac{b}{g}\times\frac{a}{g}=\frac{ab}{g^2}.$$

The length of a single turn of the wire in the first or inner layer is $2\pi\frac{c+g}{2}$, and for the outermost $2\pi\frac{c+2a-g}{2}$, and consequently the total length of these layers is $\frac{b}{g}.\,2\pi\frac{c+g}{2}$ and $\frac{b}{g}2\pi\frac{c+2a-g}{2}$.

The intermediate layers form with these two an arithmetical progression of which the above expressions are the extreme terms, and the number of terms is $\frac{a}{g}$. The sum of this series, or the total length of the wire, will be given by the formula:

$$(2) \qquad H=\frac{b}{g}\,\frac{2\pi(c+g+c+2a-g)}{4}\,\frac{a}{g}$$
$$=\frac{\pi ba(a+c)}{g^2}.$$

The values of t and H are thus obtained in terms of the different elements entering into the construction of an electro-magnet.

These formulas are useful, as they enable us to determine the length and number of turns of the wire of a helix, from the dimensions of the wire, and the thickness and length of the helix.

An expression for the electro-magnetic force is obtained by starting from the laws of Jacobi, Dub and Müller, which express the value of the force F of an electro-magnet or its magnetic moment, in terms of the intensity I of the electric current and the number of turns of the helix; the formula being

$$F=\frac{Et}{R+H};$$

and the value of the attractive force A by the square of this expression; or

$$A=\frac{E^2t^2}{(R+H)^2}.$$

By substituting for t and H in these formulas the values previously obtained, we get:

$$F=\frac{Eab}{Rg^2+\pi ba(a+c)}$$

and

$$A=\frac{E^2a^2b^2}{[Rg^2+\pi ba(a+c)]^2}.$$

The formulas enable us to determine different conditions of maximum effect according as we change the values of a, b, c and g. These conditions are dependent on, 1st, the resistance to be given to the helix; 2d, the ratio between the size of the helix and the magnetic core; and, 3d, the dimensions of the electro-magnet itself.

The different parts of the circuit are composed of different conductors, and it becomes necessary in determining the resistance to consider the wire of the helix, apart from its covering. As g represents the diameter of the covered wire, we may take $\frac{g}{f}$ for the diameter of the naked wire; f having a value of 1.6 for very fine wire, and 1.4 for the medium sizes.

Now in modifying the value of R in the formula so as to involve the above condition, the different conducting powers of the helix and the remainder of the circuit must be taken into account. If q be taken to represent the ratio of conductivity, we shall have for the modified value of R the expression $\frac{qRg^2}{f^2}$. The unit section of conductivity being

$$0^m.000016.$$

Now if the diameter $\frac{g}{f}$ of the wire of the helix is made to vary while the thickness of the helix remains constant, the length will vary also, so that the resistance of the helix will vary inversely as g^4.

As the value of H in the preceding formulas represents the resistance of the helix, the denominators of the values of F and A become:

$\frac{q\mathrm{R}g^4+\pi ba(a+c)f^2}{f^2g^4}$ and $\left(\frac{q\mathrm{R}g^4+\pi ba(a+c)f^2}{f^2g^4}\right)^2$

and the values themselves:

$$(3)\quad \begin{cases} \mathrm{F}=\dfrac{f^2g^2\mathrm{E}ab}{q\mathrm{R}g^4+f^2\pi ba(a+c)}, \text{ and} \\ \mathrm{A}=\dfrac{f^4g^4\mathrm{E}^2a^2b^2}{[q\mathrm{R}g^4+f^2\pi ba(a+c)]^2}. \end{cases}$$

II.—Conditions of Maximum Effect of Electro-Magnets on a Simple Circuit.

1st. Conditions as determined by the resistance of the helix.

The values of A and F as given above may be discussed from different points of view. The conditions of maximum effect, for instance, may be required for an electro-magnet of *fixed dimensions*, and it is desired to adapt its resistance to that of a given resistance R of the exterior circuit; or it may be required to adjust the resistance of the helix to that of the external circuit without being restricted in the dimensions of the helix. In the first case the variable quantity is the diameter g of the helix wire, and in the latter it is the thickness a of the helix itself.

When we consider that in the preceding formulas the diameter of the helix wire is not g, but $\frac{g}{f}$, in which f is a constant, we see that the calculation is not as simple as it appears at first sight, and for this reason, those who first employed these formulas neglected the value of f and employed g to represent the true diameter of the conducting wire. In taking into account the value of f, we shall not be able always to consider it constant while g is variable, and it is evident that on some occasions it will be necessary to regard this fact. But assuming only the more simple conditions of working, we easily determine from the preceding formulas that the conditions of maximum effect correspond to the equation

$$\frac{q\mathrm{R}g^3}{f^3}=\frac{\pi ba(a+c)}{g^3};$$

that is to say, R=H; which signfies that for electro-magnets of the same dimensions, having spools or helices of the same diameter, *that size of wire for the helix is most effective which renders the resistance of the helix equal to that of the exterior circuit.*

If we take the thickness of the covering of the wire into consideration, the formula proves that the most effective helix *is that whose resistance is to the resistance of the exterior circuit, as the diameter of the naked wire is to the diameter of the covered wire.*

If the thickness a of the helix be made to vary, and suppose the action of the spirals to remain the same (an admissible hypothesis under ordinary conditions when we take into account the difference of resistance arising from their greater distance from the soft iron core), the conditions of maximum effect as exhibited in the formula properly modified indicate that R should be equal to $\frac{\pi ba^2}{g^2}$. That is to say, equal to the length H of the wire of the helix divided by the ratio $\frac{a+c}{a}$, or what amounts to the same thing,

$$\mathrm{H}=\mathrm{R}\left(1+\frac{c}{a}\right).$$

Translated into ordinary language, this deduction signifies that, among several electro magnetic helices wound with wire of the same size, but having a different number of spiral turns, that which affords the best results upon a circuit of given resistance, *is that helix whose resistance is to the resistance of the external circuit as the thickness of the helix plus the diameter of the core is to the thickness of the helix alone.*

As in applications of electro-magnetism we generally start with a soft iron core of fixed dimensions, and as the conditions of maximum effect, of which we will presently speak, require that the thickness of the helix should be equal to the diameter of the core, and as, on the other hand, the thickness of the insulating cover varies and is thus far undetermined, it is one of the first things to be considered in the solution of the problem of construction. But for an experimenter who desires to know what resistance a circuit should have in order to produce the best effect with a given electro-magnet, the conditions explained in the following section indicate that the resistance of the external circuit should be *less than half that of the elec-*

tro-magnet, if the thickness of the helix is equal to the diameter of the core, as it should be.

2d. Conditions of maximum effect depending on the ratio between the thickness of the helix and the diameter of the iron core.

A second very important point in construction of electro-magnets is the determination of the limit of thickness to be given to the spools of wire to insure the best results. It is understood in a general way that the force of electro-magnets augments with the diameter of the core, and the resistance of the helix increasing in consequence of this increase of diameter, it is evident that there must be a limit at which the advantages gained by increasing the size of the core are counterbalanced by the increase of resistance in the helix. This limit may be determined by calculation.

In equations (3) expressing the values of the magnetic moment and the attractive force, F and A, we will make the quantity c (the diameter of the soft iron core) to vary, and establish an algebraic relation between it and a, the thickness of the helix. This is easily done, since we can suppose the helix wound directly upon the core, and its interior diameter will then be$=c$; and we shall be able in placing the electro-magnet in a condition of maximum effect in relation to the resistance of the exterior circuit, to obtain an expression susceptible of a maximum value, and the ratio of R to H will also be one or the other of those values established in the previous sections.

In representing by λ the coefficient by which to multiply the length of the helix to place the total circuit in one or the other of the conditions of maximum, and supposing the thickness of the helix a and consequently the number of turns of the wire t invariable; the attractive force A and the magnetic moment F have for values, according to the law of Müller relative to the increase of force with the diameter of the iron core,

$$(4)\quad F=\frac{g^2E\sqrt{c}}{\lambda\pi ba(a+c)} \quad \text{and}$$

$$A=\frac{g^4E^2c}{[\lambda\pi ba(a+c)^2]^2}$$

The expressions become maximum values for a certain value of c, but the quantities R and H are supposed to vary at the same time in proportion as the helix is elongated by reason of the increase of the magnetic core.

If we take the derivative of the preceding expression in relation to c considered as a variable, and equate it to zero, we find that the maximum corresponds to the condition $a=c$. That is, the thickness of the helix equals the diameter of the soft iron magnet. Now this is just the conclusion demonstrated by the experiments of the writer, also the conclusion to be reached at the end of this essay.

The advantages of the laws stated above are readily seen, as they enable us to simplify the calculations in determining the relative dimensions of the parts in constructing electro-magnets. The formula for the length of the helix wire, for instance, becomes, under the conditions for maximum effect, $=\frac{2\pi bc^3}{g^2}$ and if we regard the length b of the electro-magnet as a function of its diameter c, making $b=mc$, the above expression becomes

$$(5)\quad \frac{2\pi c^3m}{g^2} \text{ or } \frac{75.4c^3}{g^2}$$

making $m=12$ for both branches of the magnet, a proportion which accords well with good practice.

In this formula only two quantities, c and g, are required to be known, and which may be determined by means of relations which will be given further on. On the other hand, we have for the value of the number of turns of the wire in the helix,

$$(6)\quad \frac{12\,c^3}{g^2}.$$

When the electro-magnet satisfies the conditions of maximum effect, R$=$H, $a=c$, and $b=cm$; as g is indeterminate it is necessary that R should be made a function of g. This is accomplished by putting the value of H equal to the length of wire required for R; whence

$$\frac{2\pi c^3m}{g^2}=\frac{q\mathrm{R}g^2}{f^2}$$

from which we get

$$g^4=f^2\frac{c^3}{\mathrm{R}}\cdot\frac{2\pi m}{q},$$

and as the last fraction is composed of known terms, this value reduces to *

$$(7) \quad g=\sqrt{f\sqrt{\frac{c^3}{R}}\cdot 0.00020106}$$

3*d*. *Conditions of maximum effect depending on the length of the iron core.*

From the deductions already made it is easy to see that it is a matter of some importance to make the length of the magnet bear a different relation to its diameter. But the question that remains yet to be answered is—can we determine this length by calculation? It is certain that if we consider the length b only, no condition of maximum effect can be deduced from the values of F. and A. For if, according to the law of Müller, the forces increase as the square root of the length of the magnetic core, these formulas are not susceptible of maximum values when b varies. But if we make b a function of the diameter c the attractive force becomes proportional to

$$c\times\sqrt{cm} \text{ or } c^{\frac{3}{2}}$$

and we obtain for the value of A, taking the other conditions of maximum value into consideration:

$$(8) \quad A=\frac{E^2m^2c^4c^{\frac{3}{2}}}{[Rg^2+2\pi c^3m]^2}$$

in which R is supposed expressed in terms of a given length of wire of the helix. Now from this manner of reasoning we can readily see that there should be a limit to the value of m, for the magnetizing helix having a giving resistance relative to the external circuit, and this helix having a thickness equal to the diameter of the magnetic core, the resistance can be made to vary more or less according to the ratio between the diameter and length of the core upon which the helix is wound. As the electro-magnetic force increases with the diameter of this core, there is an advantage in increasing this diameter up to a certain point; but on the other hand, as the number of turns of wire for a given length diminishes, as this diameter increases, it is preferable perhaps not to enlarge the diameter but to increase the length of the core. As this requires an increased number of turns of the wire, it compensates under certain conditions for the smaller diameter of the core. The theoretic formulas indicate the limit only indirectly for the conditions of maximum deduced by the preceding formula, taking c as the variable, give $\frac{2\pi c^3 m}{g^2}=11$ R. That is to say, according to the hypothesis admitted, *we can increase the dimensions of the magnetic core until the resistance of the magnetizing helix is eleven times the resistance of the exterior circuit.*

Under these conditions we have

$$m=11\frac{Rg^2}{2\pi c^3},$$

and we see that m becomes eleven times the ratio of resistance of the the exterior circuit to that of the helix when the diameter of the magnetic core equals its length; for in this case this helix has for its expression $\frac{2\pi c^3}{g^2}$; now, as the ratio of the resistance R to that of the helix should be = 1 to satisfy the conditions of maximum effect previously found, we deduce, from the preceding formula, $m=11$.

In the calculations published in *Recherches experimentales sur les maxima electro-magnetiques*, by the writer, these conclusions are demonstrated.*

* In this expression q represents the ratio of conductivity or of resistances between R and H, R being expressed in meters of telegraph wire. As this wire is iron, and the helix is copper, the ratio is about 6. On the other hand, the diameter of the telegraph wire is 4 millimeters, and its cross-section is 0.m000016; q therefore equals $\frac{6}{0.000016}=375000$, and $\frac{2\pi m}{q}=0.00020106$ as above.

* Suppose, for example, three electro-magnets in a circuit of 64 meters of telegraph wire, the magnets having diameters respectively of 0.m008, 0.m007 and 0.m006, with lengths eleven times their diameters.

The first of these, is furnished with a helix of No. 12 wire, 0.m0006 in diameter, would have a helix 99 meters in length and a resistance represented by 1056 meters. The second magnet would have a helix with 66 meters length of wire, and 704 meters of resistance. The third 41.m36 for length of helix, and a resistance of 441 meters.

The numbers of terms of spiral deduced from the formula $\frac{11c^2}{g^2}$ is 1955 for the first magnet, 1497 for the second, and 1100 for the third. The $\frac{3}{2}$ powers of the diameters being 0.000715, 0.000585 and 0.00046 respectively, we find for the forces of the magnets 0.247868, 0.252891 and 0.250307. The advantage here is plainly with the second magnet, whose resistance is 704 meters, or eleven times that of the exterior circuit. But this is no longer the case if R be made equal to 704.m. The forces then become 0.10087, 0.07524 and 0.04871, and it is clearly the larger magnet that has the advantage, because it is in this one that the resistance is so great compared with R, that this latter value does not affect so largely the value $(R+H)^2$ the divisor of $t^2c^{\frac{3}{2}}$.

As the helices are generally furnished with a disk or spool-end, and the core projects slightly, and as at the lower or connected end a little space is needed for the connections, the coefficient 11 should be slightly increased. Experience indicates that it should be raised to 12.

III.—Conditions of Maximum on Derived Circuits.

The deductions made in the preceding chapter suppose a permanent current established; that there is no reaction arising from extra currents; that the iron of the electro-magnet is in a condition of magnetic saturation, so that the laws of Dub and Müller are applicable, and finally that the exterior circuit R is perfectly insulated. When these conditions do not exist, the above conclusions are not valid, and calculation proves that the resistance of the helix should be considerably reduced. The experiments of M. Hughes have demonstrated this. And these have been furthermore confirmed by Lenoir on a telegraph line between Paris and Bordeaux, which required very rapid alterations of magnetizing and demagnetizing.

It is impossible therefore to establish for electro-magnets a formula which shall express exactly the conditions of maximum to the resistance of helices; but considering the conditions admitted in the preceding formula, and allowing as experiment shows, that the magnetic attraction increases in a ratio more rapid than the square of the intensity of the current, when the iron core is not saturated, we may conclude, as we shall see further on, that from the circumstance alone of rapid charging and decharging the magnet, that the resistance of the helix should be considerably diminished. But a still greater reduction is called for by the action of derived currents on the lines. A telegraph line of 500 kilometers from Paris to Bordeaux required a reduction in the resistance of the electro magnet to 40 km.

All the deductions we have thus far established are, therefore, at fault in such applications of electro-magnetism. But such is not the case in the mechanical applications where the circuit is protected, where the magnetism can be freely developed in the iron core, and where the iron is selected with reference to its capacity for saturation.

We shall have occasion presently to examine the condition of the magnetic core, but in order to have done with the laws previously stated, we will consider for a moment the conditions of maximum effect of an electro-magnet introduced into one of the parts of a derived circuit.

In considering the most simple case, that of a single derivation u established upon a circuit whose resistance is l with a common resistance R starting from the battery, we shall find that the attractive force of the electro-magnet interposed upon l will be:

$$A=\frac{E^2u^2t^2}{[R(u+l+H)+u(l+H)]^2}$$

And if we substitute for t and H their values previously found, we arrive at an expression, considering g as a variable, which corresponds to the conditions of maximum:

$$(9)\qquad \frac{qg^2}{f^2}\left(l+\frac{Ru}{R+u}\right)=\frac{\pi ba(a+c)}{g^2}$$

Causing the thickness a of the helix to vary while g remains constant, the conditions of maximum are represented by:

$$\frac{qg^2}{f^2}\left(l+\frac{Ru}{R+u}\right)=\frac{\pi ba^2}{g^2}$$

In the first of these equations, the second member represents the resistance of the helix, while the first member is the total resistance of the exterior circuit expressed in units of the same order as those giving the length of the wire of the helix. But this *total* resistance is not an increased value. It is represented in fact by

$$R+\frac{lu}{l+u}.$$

The total resistance here should be considered as if the part common to both derived currents were represented by l and as if the really common part R were only a derivation.

In the second equation the first number represents as before the total resistance of the circuit taken inversely; but this total resistance considered as the resistance of an insulated line, should be less than that of the helix in the proportion of 1 to $1+\frac{c}{a}$ in order to satisfy the conditions of the maximum effect.

We may conclude then that the laws of maximum electro-magnetic action on circuits subjected to derivations, are the same as those relating to simple circuits, by supposing that the resistance R stands for the total exterior resistance, with its branches, and admitting that this total resistance is considered *as though a battery were substituted in the circuit of the electro-magnet.* Now as the total resistance of a circuit thus branched is less than the resistance of the original line, the helix should have a less resistance than this latter. All the formulas have been verified by experiment, as we shall see.

When the derived circuits have a feeble resistance, and when the elements of the battery may be arranged at pleasure, calculations will lead to a conclusion analogous to that just reached, but of a converse kind. It is when the combination of cells is such as to make the battery resistance equal to the total resistance of the circuit, that the best results are obtained. But if after establishing a permanent arrangement of the battery, a series of branches are started from the poles of the battery, and the maximum effect upon the separate circuits is sought, as often happens in practice, the conditions of resistance of electro-magnets interposed in the derived circuits are quite different from those heretofore considered. In this case the magnets, in place of having a resistance less than that of the battery, will have a greater resistance, and greater also in proportion to the number of branches. This is easily understood when it is considered as was said above, that the resistance in the battery should always be equal to the sum total of the derived circuits, and that this total resistance becomes less in proportion as there are more branches, so it is necessary to increase the resistance of each individual branch to compensate for the number. If the derived circuits, with their electro-magnets, were all equal, this increase would be proportioned to their number, for the formula for intensity of the current on each branch would then become

$$\frac{n\mathrm{E}}{xn\mathrm{R}+\mathrm{H}},$$

in which x represents the number of branches. But if the derived circuits are of unequal resistance, and if b stand for the number of cells united for *quantity*, and a the number joined for *tension*, the preceding formula will become for two branches, u and H:

$$\frac{a\mathrm{E}}{\frac{a}{b}\mathrm{R}\left(1+\frac{\mathrm{H}}{u}\right)+\mathrm{H}},$$

and the conditions of maximum correspond to

$$\frac{a}{b}\mathrm{R}\left(1+\frac{\mathrm{H}}{u}\right)=\mathrm{H}, \text{ or to } \mathrm{H}=\frac{a\mathrm{R}u}{ub-a\mathrm{R}}.$$

When $u=\mathrm{H}$ then $\mathrm{H}=2\mathrm{R}\frac{a}{b}$. We shall find later an application of these principles.

IV.—Application of the Laws of Maxima to the Construction of Electro-Magnets.

The different laws and formulas which we have established enable us to readily solve the problems relating to electro-magnetic attractions. But it becomes necessary here to introduce the law of Müller relative to the saturation of magnets; a law which establishes that in order to develop in two electro-magnets the same fractional part of their maximum force, it is necessary that the products of the intensity of the currents which charge them multiplied by the number of turns in their helices respectively, should be to each other as the $\frac{3}{2}$ power of their diameters.

This may be thus formulated:

$$\frac{\mathrm{I}\ell}{\mathrm{I}'\ell'}=\frac{\sqrt{c^3}}{\sqrt{c'^3}}$$

It is easy to see how, from this formula, it becomes easy to determine the required conditions for an electro-magnet of given diameter or power, not only for furnishing all the force of which it is susceptible, but so that the laws of Jacobi, Dub and Müller shall be applicable. It will suffice if two of the terms in the last formula be determined by experiment. There can be obtained from a *standard electro-magnet* in which the power is varied by varying the current, and the forces produced are proportioned to the squares of the intensities of the current.

Experiments made by the writer with an electro-magnet, the iron core of which had a diameter of 0.m01 and the helix a

resistance of 200km, and which was placed in a circuit of 118600m, it was found that the ratio in question could be obtained when the battery consisted of 20 Daniell cups. As the conditions of this magnet were known, it was easy by means of the formulas which have been given to establish the constants, and from that time to have terms of comparison to be used in calculations.

Now we will proceed to show how the preceding equation united with the equations

$$t=\frac{mc^2}{g^2},\quad H=\frac{2\pi c^3 m}{g^2},\quad I=\frac{E}{2R},\quad R=H$$

aid us to solve the problems of which we have spoken in determining the values of the diameter c in relation to the quantities E and R.

If in the first equation we consider the accented terms to be known as belonging to the standard electro-magnet, and if we replace the quantities I and t by their values deduced from the maximum conditions previously discussed, we shall have

$$\frac{\sqrt{c'^3}}{I't'}\cdot\frac{Emc^2}{2Rg^2}=\sqrt{c^3}$$

And as the diameter g is indeterminate and should be satisfied by the condition R=H on the one hand, and $a=c$ on the other, it should be determined in terms of these two quantities. The equation

$$R=\frac{2\pi c^3 m}{g^2}$$

will serve the purpose.

Inasmuch as g is indeterminate and constantly variable, the term R should be reduced to a function of g. This is afforded by the value

$$g^2=\frac{f'\sqrt{2\pi c^3 m}}{\sqrt{qR}}.$$

Now substituting this for g^2 in the preceding equation, it becomes:

$$c=\frac{E}{f'\sqrt{R}}\left(\frac{\sqrt{c'^3 qm}}{2I't'\sqrt{2\pi}}\right)$$

in which the quantity in parenthesis is a constant which varies according to the system of measurement employed, but in which $m=12$, $q=375000$, and $\pi=3.1416$, and in which the other quantities may be regarded as given, since they relate to an electro-magnet working under known conditions.

On the other hand, in applying the same transformations to the value $\frac{\sqrt{c'^3}}{I't'}$ that we have to $\frac{\sqrt{c^3}}{It}$ we arrive at the value

$$c=c'\frac{E}{E'}\frac{\sqrt{R'}}{\sqrt{R}}\frac{f'}{f}.$$

As $\frac{f'}{f}$ is practically equal to unity, we can relieve the expression of the factor f' and consequently the formula simplifies to

$$(10)\qquad c=\frac{E}{\sqrt{R}}K$$

K being a constant with different values according to the unit adopted.

If E be expressed in terms of the ordinary standard the single Daniell cell, and if R be expressed in meters of telegraph wire, then K=0.172175 and the value is expressed in fractions of a meter.

In referring K and R to the system of British Association units, K = 0.015957.

The diameter c being known, all the other elements can be easily determined for maximum conditions by means of the formulas previously given, which express the values of g, b, t and H.

To know the value of the attractive force A, it will suffice to determine the value of $I^2t^2c^{\frac{3}{2}}$, remembering that $I=\frac{E}{2R}$ and that the values of t and c are given by the formulas (6) and (10); and that to get this force expressed as weight it is necessary to refer to the standard electro-magnet which gives for a constant 0.0000855.*

The value becomes

$$(11)\qquad A=\frac{I^2t^2c^{\frac{3}{2}}}{0.0000855}\text{ grams.}$$

The important results deduced from formula (10) may be thus enumerated:

* This coefficient is the force of the standard magnet as expressed by the above formula, measured by grams; the force for a distance of 1 millimeter being 26.85 grams.

1. For equal resistances on the circuit, the diameters of electro-magnets of maximum effect should be proportional to the electro-motive forces employed.
2. For equal electro-motive forces, these diameters should be in the inverse ratio of the square roots of the resistances of the circuit and battery.
3. For equal diameters, the electro-motive forces should be proportional to the square roots of the resistances of the circuit.
4. For a given electro-magnetic force, and with electro-magnets under conditions of maximum effect, the electro-motive forces of the batteries should be proportional to the square root of the resistances of the circuit. (See formula 12.)

The preceding formulas also lead to the easy solution of many problems which frequently present themselves in practical applications of electricity, and particularly in calculating directly the force of a battery, and the dimensions of an electro-magnet to afford a given attractive force on a circuit of given resistance. It is true that the results obtained are not always in accord with the calculation, by reason of the nature of the iron core which may be more or less suitable for ready magnetization, and by reason of the more or less complete saturation of the magnet; but we can always get a fair approximation, and that is considerable gained.

In order to solve the problem in question, we will determine first the fourth deduction above mentioned, and proceed to demonstrate it. This is to determine a value of the electro-magnetic attractive force in terms of the electro-motive force of the battery and the resistance of the circuit.

If we consider the expression $I^2 t^2 c^{\frac{3}{2}}$ which represents this force, we find that by substitutions* in the values of I, t and c, it becomes

$$\frac{E^{\frac{3}{2}}}{R^{\frac{9}{4}}} \cdot \frac{Q}{f^2}$$

in which Q is a constant equal to 2228 (the Daniell cell being the unit of electro-motive force, and the resistances being given in meters of telegraph wire).

Now, in estimating the value of the attractive force, we arrive at the relations

$$(12) \qquad \frac{E^{\frac{9}{2}}}{E'^{\frac{9}{2}}} = \frac{R^{\frac{9}{4}}}{R'^{\frac{9}{4}}} \text{ or } \frac{E}{E'} = \frac{\sqrt{R}}{\sqrt{R'}}$$

As in the values of E and R the numbers n and n' of the elements are found, we can easily calculate these numbers, knowing the values of the constants e and ρ of the element employed, for we have from the preceding equation:

$$\frac{ne}{n'e'} = \frac{\sqrt{n\rho + r}}{\sqrt{n'\rho' + r'}}$$

and if the accented quantities relate to those of the standard electro-magnet which are known, it is easy to deduce from the preceding formula the value of n, as it only requires the solution of an equation of the second degree with one unknown quantity.

If a liquid body sends vapor into an unlimited atmosphere there will proceed from each element of its surface, during a unit of time, a quantity of vapor proportional to an electric charge which is present and in equilibrium upon the element. *Les Mondes* says the lines of the vapor currents correspond to the lines of electric force, and the surfaces of equal vapor pressure to the surfaces of equal potential. The electric equilibrium of an infinitely small circular or elliptic plate is the electrostatic analogue of the problem of the evaporation of a liquid contained in a basin of circular or elliptic contour.

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* The substitutions referred to are:

$$I^2 = \frac{E^2}{4R^2}.$$

$$t^2 = \frac{E\sqrt{R}}{f} \quad 123903.$$

$$I^2 t^2 = \frac{E^2}{f^2 R^{\frac{3}{2}}} \quad 30976.$$

$$c^{\frac{3}{2}} = \frac{E^{\frac{3}{2}}}{R^{\frac{9}{4}}} \quad 0.07195.$$

$$A = I^2 t^2 c^{\frac{3}{2}} = \frac{E^{\frac{9}{2}}}{f^2 R^{\frac{9}{4}}} \quad 2228.$$

WATER-JET PROPELLERS.

By W. H. WHITE.

From "Nature."

VERY early in the history of steam navigation, attempts were made to employ the "hydraulic" or "water-jet" propeller. About 1782 Rumsey began to work in this direction, using a steam-engine to force water out at the stern of a boat, the inlet being at the bow. His experiments are said to have extended over twenty years, but led to no practical result. Another American, named Livingston, applied the same principle of propulsion in a different manner. A horizontal wheel, or turbine, was placed in the bottom of the boat, near the middle of the length, the water was admitted from beneath it, and expelled from the periphery of the wheel through an opening at the after part of the boat. In 1798 a monopoly was granted to Livingston for twenty years by the State of New York, on condition that within a given period he produced a vessel capable of attaining the speed of four miles an hour. This condition was not fulfilled, and, as is well known, the first successful steamers built in this country or abroad were propelled by paddle wheels. This form of propeller alone was employed for nearly forty years, during which period steam-ships increased greatly in numbers, size and speed, proving themselves well adapted not merely for service on inland and coasting navigation, but also for ocean voyages. Just when the Transatlantic steam service had been successfully commenced by the *Great Western* and *Sirius*, both paddle steamers, the screw-propeller began to threaten the supremacy of the paddle-wheel; and the success of the *Archimedes* in 1840 led to the adoption of the screw in the *Great Britain*, as well as the construction of the screw sloop *Rattler* for the Royal Navy. Soon after came a revival of the water-jet propeller by the Messrs. Ruthven of Edinburgh. In 1843 their first vessel was tried, attaining a speed of about seven miles an hour. Ten years later a fishing-vessel was built on the same principle, and exceeded nine miles an hour. Several other river steamers and small craft were constructed with jet-propellers in the period 1853-65, but they were all comparatively slow, and the plan did not grow into favor either as a substitute for the paddle-wheel or the screw.

There were certain features in the jet-propeller which recommended it to the judgment of many naval officers who had witnessed the trials of vessels so fitted; their influence led the Admiralty in 1865 to order the construction of a small armored vessel, appropriately named the *Waterwitch*, which was to be fitted with Ruthven's propeller. Admiral Sir George Eliot was one of the principal advocates of a trial of the new system, in which he has always continued to take a great interest. In the German navy, trials of the Ruthven system have also been made on a small vessel named the *Rival*, and experiments of a similar nature have been made in Sweden. At the present time Messrs. Thornycroft are building for the Admiralty a torpedo boat, to be propelled by water-jets, the trials of which are awaited with interest, since they will furnish another comparison between the performances of the hydraulic propeller and the screw.

The Ruthven system agrees in its main features with the proposal made by Livingston forty years earlier. As an example the arrangements of the *Waterwitch* may be briefly described. Openings are made in the bottom of the ship amidships, to admit the water into a powerful centrifugal pump or turbine, the axis of which is vertical. The main engines drive the turbine, expelling the water with considerable velocity through curved pipes or passages leading to the "nozzles" placed on each side at the level of the water-surface. When the vessel is going ahead the jets are delivered sternwards; if it is desired to move astern the engines are not reversed, but valves are operated in the outlet pipes, and the jets are delivered through

the forward ends of the nozzles. These motions of the valves can be made from the deck by an officer in command. If desired, the jet on one side can be delivered ahead, and that on the other side astern, the vessel then turning without headway. This power of control over the movements of the vessel, without reversing the engines, is one of the chief advantages claimed for the system; and it is undoubtedly of value, especially in war-ships. Another advantage claimed for the jet-propeller is the power of turning it on an emergency, into a powerful pump, by which large quantities of water can be discharged from the interior of a ship that has been damaged in action. This latter feature cannot be regarded as of primary importance, however, seeing that modern war-ships are minutely subdivided into water-tight compartments, and must depend for their flotation upon the integrity of the bulkheads and other partitions, if their skins have been broken through by ramming or torpedo explosions. A further claim on behalf of the jet-propeller for war-ships is based upon the less risk of disablement in action, as compared with screws or paddle-wheels; and this claim may be admitted. On the other side must be set the fact that all the trials made hitherto in vessels fitted on the Ruthven system have shown a less speed for a given amount of engine-power than would have been obtained with the screw-propeller. It may be urged, of course, that the decrease in speed should be accepted, at least in special cases, in order to secure the undoubted benefit of the hydraulic system. But the general feeling of naval architects and marine engineers is in favor of the use of twin-screws rather than water-jets for war-ships, the duplication of machinery and propellers decreasing the risk of disablement, giving great maneuvering power, and securing higher speed than could be obtained with the jet propeller.

Recently further trials have been made with a vessel built in Germany, from the designs of Dr. Fleischer, who claims to have devised a novel and more efficient system of hydraulic propulsion. A brief notice of the invention appeared in *Nature*, vol. xxvi., p. 18,; fuller details are to be found in two pamphlets published by the inventor: "Der Hydromotor," and "Die Physik des Hydromotors" (Kiel, 1881). The first of these pamphlets contains a general description of the system, as applied in the *Hydromotor* (a vessel of 110 feet in length, and about 100 tons displacement), a summary of her trials, compared with those of earlier vessels engined on Ruthven's system, and an enumeration of the advantages to be obtained by using jet-propellers instead of screws or paddles. The second pamphlet contains a statement of the experimental and mathematical investigations conducted by Dr. Fleischer in working out his system.

Dr. Fleischer dispenses with a turbine, and allows the steam to act directly upon the water in two large vertical cylinders placed amidships. These two cylinders communicate with the ejecting nozzles which are situated on either side of the keel. In each cylinder there is a "float" or piston of nearly the same diameter as the cylinder, with a closed spherical top; when this float is in its extreme upper position, the cylinder is full of water. Steam is then admitted into the upper part of the cylinder above the float, the latter is pressed down, and the water is expelled through the nozzle-pipe with great velocity. At a certain portion of the stroke, the admission of steam is shut off automatically, the remainder of the stroke being performed during the expansion of the steam, and the velocity of ejection of the water gradually diminishing. At the conclusion of the stroke, the exhaust-valve from the steam space to the condenser is opened, the steam rushes out, forming a partial vacuum above the float, and the water enters, pressing the float up. The entry of the water at this stage is partly through the nozzle, and partly from a separate valve communicating with the water-space of the surface condenser. In order to utilize the vacuum as much as possible, and to increase the effective "head" of water during the down stroke, the cylinders are placed as high as convenient in the vessel. Two cylinders acting alternately were used in the *Hydromotor*, for larger or swifter vessels it is proposed to use a greater number of similar cylinders. As in other jet-propelled vessels valves operated from the deck enable the commanding officer to reverse the direction of outflow of either or both jets, making the vessel move ahead or astern, or turn on

her center. The position of the nozzles in the *Hydromotor* is not so favorable to maneuvering power as in the *Waterwitch*, and the difference in behavior is likely to be appreciable.

Greater interest attaches to the trials of speed than to those of turning. Unfortunately the records are too meager to enable a decisive opinion to be formed on the merits of the new system as compared with that of Ruthven. Dr. Fleischer claims that the *Hydromotor* attained a speed of 9 knots with 100 indicated horse-power; but the conditions under which this speed was attained may have differed considerably from those under which measured-mile speed trials are conducted in this country. Any exact comparison of the performances of two steam-ships with either similar or different systems of propulsion, demands as its basis the elimination of all varying conditions, the determination of the true mean speed, and the calculation of the engine power corresponding to that speed. Dr. Fleischer may have done all this, but it does not clearly appear in his publications whether he has or not. He distinctly claims for his system a very high "efficiency" as compared with that of Ruthven, but it will be shown hereafter that the formula which he uses is not absolutely correct; and what is more important to note is the circumstance that Dr. Fleischer clearly does not possess the experimental data respecting the resistance offered by the water to the motion of the *Hydromotor* when towed at various speeds, which would enable him to express the true efficiency of the propelling apparatus. On this point a few further remarks may be permitted.

Supposing a vessel to be towed at any speed, and her resistance to be ascertained by a dynamometer, the horse-power expended in overcoming that resistance can be calculated, and, in the terminology of the late Mr. Froude, is styled the "effective horse-power." Next let it be supposed that the vessel is driven at the same speed by her own machinery, and that the "indicated horse-power" in the cylinders is ascertained. The ratio of the "effective" to the "indicated" horse-power expresses the true efficiency of the propelling apparatus, excluding from the account, of course, the efficiency of the boilers. Now what has been said above respecting Dr. Fleischer's figures simply amounts to this: he does not appear to have ascertained the effective horse-power of the *Hydromotor*, and consequently cannot express the true efficiency except as an estimate.

The excess of the indicated horse-power over the effective in any steam-ship is to be accounted for by the waste work of the mechanism, the waste work of the propellers, and the "augment" of the tow-rope resistance produced by the action of the propellers. In good examples of screw steamers the effective horse-power at full speed has been found to vary from 40 to 60 per cent. of the indicated power. Dr. Fleischer claims for the *Hydromotor* a corresponding efficiency of about 34 per cent. at full speed; but not, it would seem, with any certainty.

Passing by this comparison with screw-propelled ships, the *Hydromotor* may be compared with the *Watewitch*. She gains upon the latter obviously in the avoidance of much waste work in the mechanism. In the Ruthven system there is necessarily more waste work in the engines which drive the turbines, and in the friction of the water in the turbines and passages to the nozzles, than has to be incurred in the Fleischer system. On the other hand, in the latter system, there must be some loss from condensation of steam in the cylinders, and the high mean velocity of ejection must be a disadvantage. The considerable variations in the velocity of ejection at different parts of the stroke must also be a disadvantage, as compared with the uniform velocity of delivery from a turbine. Respecting the condensation it is asserted, as the result of experiment, that the losses are exceedingly small, the cylinders being wood-lined, and a layer of hot water being formed below the float. Experienced engineers were scarcely prepared for this satisfactory result, anticipating that more serious losses would occur from the alternate heating and cooling of the cylinders. Of course, experience in such a matter is a true test; but it is to be observed that the *Hydromotor* appears to have very ample boiler power in relation to the indicated horse-power assigned to her maximum speed. Losses from condensation cannot be estimated from the statement

of indicated horse-power. The indicator diagrams which have been published, show a very good performance.

The varying rate of outflow through the nozzles must be a source of disadvantage in the Fleischer system. For the hydromotor it is stated that the *mean* velocity of outflow was about 66 feet per second when the speed of the vessel was about 15 feet per second. We are not informed what was the maximum velocity of outflow; the minimum velocity is said to have exceeded the speed of the vessel. This varying velocity, of course, carries with it a varying thrust, and the hydromotor in this respect must be less favorable to uniform motion of the ship than the screw or paddle or Ruthven propeller, where the thrust can be kept practically constant. With two cylinders this might be more felt than with four or more cylinders, but in all cases the drawback must exist.

The high mean rate of outflow involved in the Fleischer system is contrary to the generally accepted view as to the condition most favorable to efficiency. For a given speed of ship, neglecting the augment of tow-rope resistance which may be caused by the action of the propeller, there must be a certain thrust developed, which will overcome the resistance of the water to the advance of the ship. This thrust in the jet propeller is measured by the sternward momentum generated in the jets. No matter how the mechanism may be arranged, what has to be done by it is to impart to water which has entered the ship and acquired her forward velocity, a sternward momentum which shall have a reaction equal and opposite to the fluid resistance. Momentum, it need hardly be explained, involves the consideration both of the weight of the water acted upon and of the velocity imparted to it in each unit of time. Nor is it possible to create this momentum in the water expelled from the nozzles without doing waste work in overcoming frictional and other resistances. The magnitude of this waste work may vary greatly in different examples, and it is difficult to estimate its value apart from experiment. Hence in theoretical investigations, this waste work is usually neglected, although in practice it is of great importance.

Leaving out of account for the moment this waste work, and the possible influence upon the efficiency of the propeller exercised by the disturbance produced in the surrounding water by the passage of the ship, it may be well to explain briefly the accepted theory of the action of jet-propellers. This is done in the following equations:—

Let V = the speed of outflow of the jets from the nozzles in feet per second, v = the speed of advance of the ship, A = the joint sectional area of the nozzles in square feet, w = weight in lbs. of a cubic foot of water. Then—

Cubic feet of water acted upon per second $\Big\}$ $= A.\ V.$

Sternward velocity of jets in relation to still water $\Big\}$ $= V - v.$

Thrust, or momentum created per second $\Big\}$ $= \frac{w}{g}.A.V.\ (V-v),$

where g is the accelerating force of gravity—say 32 feet per second. For sea-water $w = 64$; so that $w \div g = 2$ (nearly). Hence

Thrust (in sea-water) $= 2A.V.(V-v).$

Under the foregoing assumptions, we also have

U = Useful work of propeller (in unit of time) $\Big\}$ = work done in propelling ship.
= Thrust × speed of ship.
$= 2AV(V-v).v.$

W = waste work in race $= \frac{1}{2}$ *vis viva*.
$= AV.(V-v)^2.$

U + W = total work of propeller $= 2AV(V-v)v + AV(V-v)^2$
$= AV(V^2 - v^2).$

$$\text{Efficiency} = \frac{U}{U+W} = \frac{2v}{V+v}.$$

From the last of these equations it is seen that the more nearly the velocity of V approaches the speed of the ship v, the nearer will the efficiency approach its maximum value, or unit. Moreover, for given values of speed of ship and *thrust*, if the difference $(V-v)$ between the speeds of outflow and advance is diminished, the area of the outlets must be correspondingly increased. That is to say, if the value of $V-v$ is diminished, the *quantity of water* (A.V) operated upon must be increased. Now, in general, it has been supposed that the inferior performance of jet-propelled vessels, as compared with screw steamers was due to the small quantities of water acted upon. In the *Waterwitch*, for example, about 150

cubic feet of water were expelled per second, whereas in the rival twin-screw vessel *Viper* more than 2000 cubic feet of water were operated upon per second. In the *Waterwitch* V=30 feet per second, and v=15.7 feet per second; so that according to the foregoing formula

$$\text{Efficiency}=\frac{2\times 15.7}{15.7+30}=\frac{31.4}{45.7}=68.7 \text{ per ct.}$$

In the *Hydromotor* V=66 feet (mean velocity) v=15.2.

$$\text{Efficiency}=\frac{2\times 15.2}{15.2+66}=\frac{30.4}{81.2}=37.4 \text{ per ct.}$$

Dr. Fleischer adopts the foregoing equations, so far as they relate to *thrust* and *useful work*, but for the *total work* he uses another formula, and it is here that we venture to think he goes wrong. According to his investigation—

Total work = ½ *vis viva* of issuing streams.
= ½ × Mass of water delivered per second × (speed of outflow)².
= ½ × 2AV × V².

Hence he writes—

$$\text{Efficiency}=\frac{\text{Useful work}}{\text{Total work}}=\frac{2AV(V-v)}{2AV\times\frac{1}{2}V^2}=\frac{2V}{v^2}(v-v).$$

In thus dealing with the total work, instead of using the expression given above, Dr. Fleischer virtually ignores the fact that the vessel is in motion ahead; and that the streams issuing from the nozzles have the velocity V only relatively to her. It is upon this questionable formula for the efficiency that his estimates above-mentioned are based. For example, in the hydromotor at 9 knots, according to Dr. Fleischer—

$$\text{Efficiency}=\frac{2\times 15.2}{(66)^2}(66-15.2)=35.4$$

per cent.

If the same formula is applied to the *Waterwitch*, at 9.3 knots—

$$\text{Efficiency}=\frac{2\times 15.7}{(30)^2}(30-15.7)=49.9$$

giving about 20 per cent. less efficiency to that vessel, than is given by the accepted formula first stated.

It has been explained that the assumptions upon which the first formula rests are not fairly representative of the conditions of practice. For example, the deduction therefrom (stated above), that it is advantageous to operate upon larger quantities of water, and to reduce the excess in speed of outflow above the speed of the ship requires an important qualification in practice. This deduction would be absolutely correct were it not for the waste-work which has to be done in giving the motion to the water; but in actual practice the growth in that waste work may exceed the gain obtained by dealing with larger quantities of water. The parallel case in a screw steamer is that wherein screws of too large diameter or too large surface may involve so much more waste work on frictional or edgewise resistances, that it is preferable to use smaller screws, which operate on smaller quantities of water, but secure a more economical expenditure of power for a given speed, or enable higher speeds to be attained with a given horse-power. In setting aside the commonly received view, and making trial of a system wherein the mean velocity of the outflowing jets is extremely great, while the quantity of water operated on is small, Dr. Fleischer has made an experiment of the greatest interest to all concerned with steam propulsion. If his figures are accepted it is obvious that his system involves much less waste work than the Ruthven system between the power indicated in the cylinders and the power accounted for in the outflowing jets. On the other hand, as we have endeavored to explain, this economy of the Fleischer system does not represent the comparative efficiency of the propelling apparatus: because the high and variable velocity of outflow must involve a considerable amount of waste work in the race. A complete comparison could only be made if in the same vessel, or in two vessels of identical form and with identical boiler power, there were fitted, first, the Fleischer hydromotor; and secondly, the Ruthven arrangement. Then with the same steam producing power a careful series of trials would settle the matter conclusively. The Swedes did something of this kind in order to compare the efficiencies of twin screws and water jets, with the result that the latter were shown to be greatly inferior. Of course it cannot be expected that Dr. Fleischer would undertake such trials unaided; on the other hand, if his system is put forward for adoption in preference

to the Ruthven system, it must, at least, be shown to be more efficient, not only in certain intermediate stages in the operations of giving momentum to the jets, but as a whole. This result does not appear to have been attained as yet, so far as can be judged from the published results of trials. The information which is accessible is not complete, and some of the proposed standards of comparison are open to doubt. It is to be hoped, however, that the zeal and ability which have been displayed already by Dr. Fleischer will be still further illustrated in the continued investigation of the capabilities of his novel system of propulsion.

COMPARATIVE TEST OF BOILERS.

MADE AT THE WORKS OF THE BRUSH ELECTRIC LIGHT COMPANY OF PHILADELPHIA.*

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

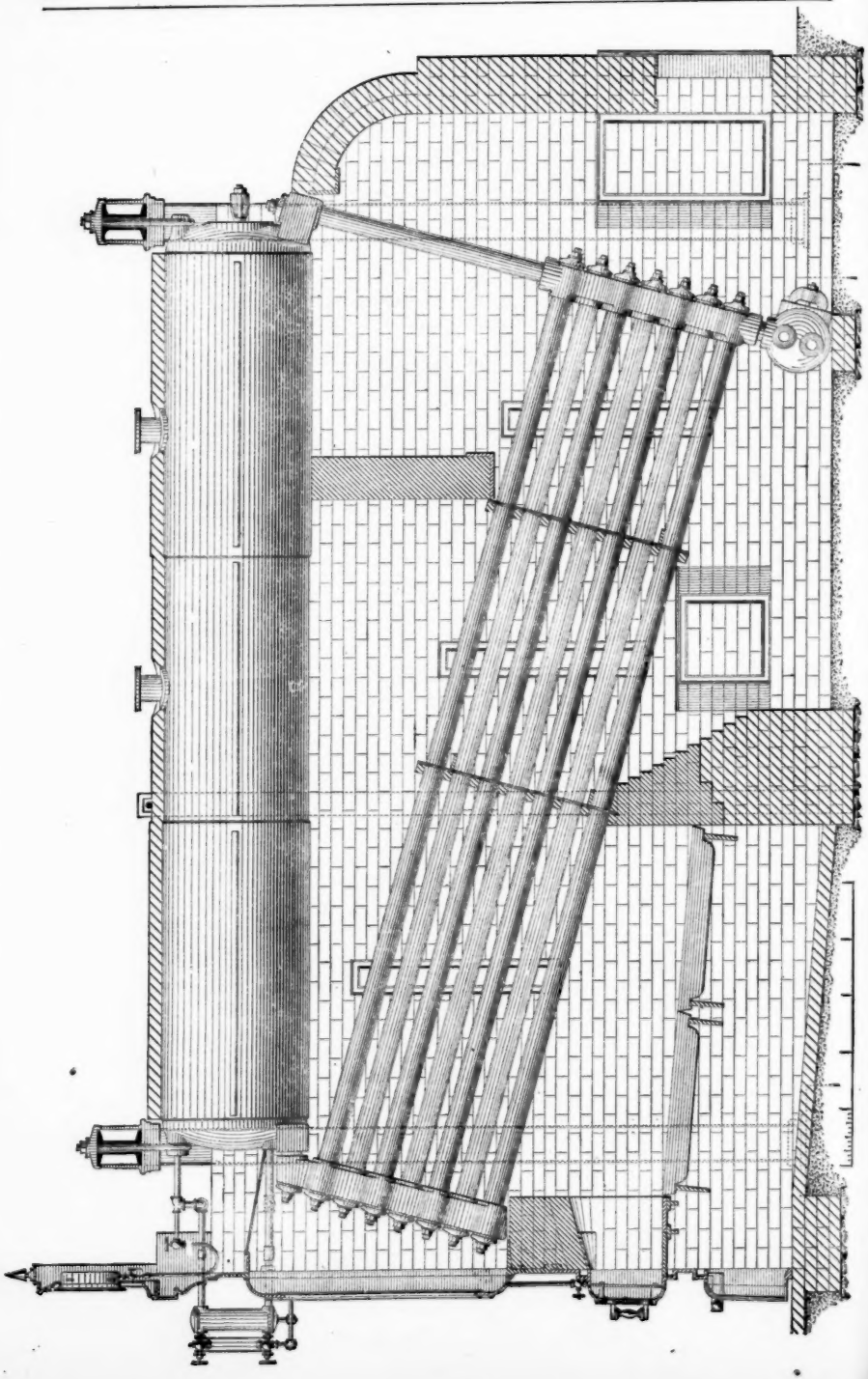
THE boiler house of the Brush Electric Light Company, where these tests were made, is situated on Twentieth street, near Chestnut street, Philadelphia. It contains ten boilers, four of the well-known sectional water-tube boilers made by the Babcock & Wilcox Company, and six return-tubular boilers, made to the specifications of the Brush Electric Light Company.

The Babcock & Wilcox boilers are set in two nests, two in each nest, but have separate furnaces, and can be used separately or in any desired combination. Each boiler contains 42 tubes 4 in. outside diameter and 16 ft. long. The arrangement of the tubes and the setting of these boilers, are too universally known to call for particular description here. The fire-grates of each boiler are 46 in. wide and 72 in. long, plain cast-iron bars, case singly, having an area of 23 sq. ft., of which about 40 *per cent.*, say 9.2 sq. ft., is open.

The heating surface is reckoned to be 800 sq. ft. in each boiler. Exclusive of some parts not very efficient, there are 796 sq. ft., giving a ratio of heating surface to grate-area of $\frac{796}{23} = 34.6$. The steam drum is horizontal, 30 in. in diameter and 16 ft. long, and the surface of the water was carried at the middle of its height, which was at 6 inches depth in the tube of the glass water gauge. The water surface for disengagement of steam is, therefore, 40 sq. ft. in each boiler. From the top of these drums, near the back end, steam is carried to the rear in separate pipes through the wall, which divides the engine room from the boiler room and enters a pipe extending the whole length of the engine room to receive steam from all the boilers and to supply all the engines. A separate pipe supplies steam to each engine, and all these pipes are, at all times, full of steam up to the starting valves of the engines, although but a small number of the engines may be in use. All steam pipes are wrapped with hair felt and covered with cloth and painted.

The six return-tubular boilers are set in one nest, and so arranged that they can be used singly or in any combination, either with each other, or with one or more of the Babcock & Wilcox Company's boilers. Each of these six boilers is 40 in. in diameter outside of the first course in front, and as each succeeding course enters the one which precedes it, (all being cylindrical), the boiler grows gradually smaller by an amount equal to twice the thickness of the iron at each of the five 40 in. courses, the last course being 37½ in. in diameter outside. Each boiler is hung in a saddle, and is set six inches lower at the rear than in front. There are in each boiler 19 flues, 4 in. outside diameter and 16 ft. 11½ in. long over the end caulking. The fire-grate is 42 in. wide and 67 in. long, making 19.54 sq. ft. area, composed of single cast-iron bars as in the other boilers, and like them, having about 40 *per cent.* of the area open, say 7.8 sq. ft. each. The heating surface, per boiler, is 428 sq. ft.,

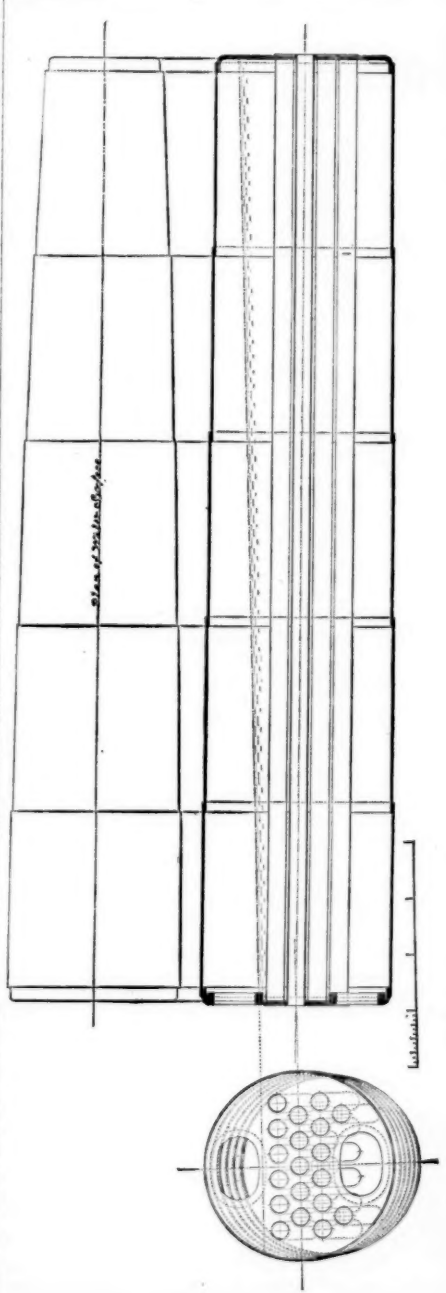
* Conducted by J. C. Hoadley, assisted by Fred. H. Prentiss, on the part of the Babcock & Wilcox Company; and Wm. Barnet Le Van, assisted by John W. Nystrom, on the part of the Brush Electric Light Company, Oct. 18, 19, 20, 23, 24, 25, 1882.



giving a ratio of heating surface to fire-grate area $= \frac{428}{19.54} = 21.9$. The usual height of the water was about 8 in. above the axis of the boiler at the front end, and about 2 in. above the top of the flues at the front. It was, also, about 2.5 in. above the lower end of the glass tube of the water gauge. The capacity and area of surface at each 1 in. of height visible at the glass water gauge, was ascertained by filling the boiler with water, which was drawn off and weighed inch by inch. The area of surface for disengagement of steam is 43.4 sq. ft. in each boiler. The inclination of the axis of the boiler and its telescopic form, combine to reduce the steam space at the rear, and for this reason steam is taken (there is no dome of any kind) from the front end, and conveyed in pipes extending over the boilers to and through the wall of the engine room, and discharged into the long steam pipe before described. The leading dimensions and ratios are collected in the following table:

COMPARATIVE TABLE.

	Babcock & Wilcox.		Return Tubular.
Heating surface, each....	796		428
Do. 2 B. & W. 3 Ret. Tub.	1592.		1284.
Fire-grate area, each	23.		19.54
Do. 2 B. & W. 3 Ret. Tub.	46.		58.62
Ratio heating to grate area	34.6		21.9
Sq. ft. per H.P., at 150 H. P	10.61		8.56
Ratio of heating surfaces.	1.24	to	1.
Ratio of grate areas.....	1.	to	1.27
Evaporat'g water-surface, each.....	40		43.4
Do. 2 B. & W. 3. Ret. Tub. sq. ft.....	80		130.2
Ratio of water-surface....	1	to	1.63



THE ENGINES.

Eight Porter-Allen engines of 40 horse-power—8″×16″, speed 285 to 295 revolutions per minute—were belted separately to eight dynamo machines, each of which when in full work supplied electricity for 40 arc lights of nominal 2,000 candle power, and occasionally an extra "test light," making 41. As already explained, the steam-pipes of all these engines were filled with steam whenever any one of them was in use, that is to say they were always filled with steam, day and night, and the condensation being constant, was a larger proportion of the steam used during the early hours of

the day—7.30 A. M. to 3 P. M.—when only 3 engines were running, than at night—7 to 11 P. M.—when 6 to 8 engines were running.

All the distribution valves, save one of those in Engine No. 4, leaked steam very badly, and the other valve of that engine leaked nearly as much as any valve in any of the other engines run during the test. This leakage was visible (in its effects) on the diagrams during expansion and compression, and was computable during exhaust, when it was pouring directly through the exhaust valve, only slightly raising the back-pressure. About 10.43 pounds of water per horse-power per hour disappeared by this leakage, during the 18th, 19th and 20th Oct., and 12.33 during the 23d, 24th and 25th, raising the consumption of water drawn from the B. & W. Co. boilers from 36.14 lbs. to 46.57 lbs., an augmentation of 28.9 *per cent.*, which gave no power, but slightly increased the resistance. That any such leakage of these valves is wholly unnecessary and gratuitous, is within Mr. Hoadley's personal knowledge, obtained by the examination of many diagrams from engines of this construction. Indeed, some of the 8 engines were put in order by the builders before the trial began, and were said to have tight valves, but none of the engines so treated except one (No. 4) were among those used during the trials, and this was again taken apart before the trial, after it was left by the builders.

The effect upon the boilers of this leakage, was to impose on them a great increase of duty, amounting to nearly one-third—and requiring them to be forced much beyond their rated capacity. They were required to evaporate, and did actually evaporate 6054 lbs. of water per hour. Without the leakage of the distribution valves, they would have been called on for less than 4307 lbs. per hour, as the entrained water would then have fallen from 3.15 *per cent.* to about 2.36 *per cent.*, a saving of 34 pounds per hour.

Leakage of steam during exhaust, not visible on diagrams; computed from the visible leakage during expansion:

Engine No. 1=339 lbs. per hour.
" No. 2=673 " " "
Engine No. 3=424 lbs. per hour.
" No. 4=263 " " "

No. 4 leaked only at one end.

Desiring that the question of the efficiency and economy of these two sets of boilers should be settled on its merits, the Babcock & Wilcox Company arranged with the Brush Electric Light Company, of Philadelphia, that a comparative test of the two sets of boilers should be made in a manner that should be satisfactory to all concerned, and conclusive as to their relative value. Accordingly, Mr. Wm. Barnet Le Van of Philadelphia was selected by the Brush Electric Light Company as their representative, and Mr. John C. Hoadley, of Boston, was chosen on the part of the Babcock & Wilcox Company.

It was further agreed that the conditions of the tests should be as nearly as possible alike in both cases, and that a double comparison should be made in the following manner:

First. A careful evaporative test of each of the two sets of boilers, embracing a determination as accurate as possible of the quality of the steam as to "dryness;" and

Secondly, A comparison of the work done by the one set with that done by the other, as shown by the indicated power of the engines.

With this, such other information was to be obtained as might be useful or confirmatory.

It was accordingly agreed between Mr Le Van and Mr. Hoadley, with the concurrence of the engineer of the Brush Light Company, that exactly similar tests should be made of the two sets of boilers, each set to be tested for three consecutive days, between the hours of 7.30 A. M. and and 2.30 P. M., during which hours only three of the engines and three dynamos would be in use, supplying 120 arc lights for actual illumination and 1 test light, making 121 in all; and one engine running with throttled steam, at less than half full speed, driving a dynamo without current, and giving no light, merely to be in readiness for immediate use if wanted for an emergency.

Such a test of the Babcock & Wilcox boilers was in fact carried out on Wednesday, Thursday and Friday, Oct. 18,

19 and 20, 1882, save that on Wednesday the 18th, these boilers continued to supply steam until 3 P. M., half an hour later than the stipulation.

On the following Monday the task of supplying the engines with steam was put upon 3 of the Return Tubular boilers, a task which they performed on Monday, Oct. 23d, until 2.30 P. M.; but on Tuesday and Wednesday, Oct. 24 and 25, on account of an unusual demand for light in the early hours of the afternoon, and the consequent starting up of the spare engine and dynamo, with 11 arc-lights for illumination and 1 test light, making 12, and 134 in all, the test on these two days was discontinued at 12 o'clock M., and embraced only 4 hours and 30 minutes each day. The Babcock & Wilcox boilers were therefore run for trial 21.5 hours on three consecutive days, and the Return Tubular boilers only 16 hours.

The coal furnished for these tests was selected and supplied by the engineer of the Brush Electric Light Company, and so far as size alone is concerned was what is usually called chestnut. The coal used on the 18th, 19th and 20th Oct. with the B. & W. boilers was *unscreened*, dull looking and extremely dirty and wet. No objection was made to it, because we supposed that it would be no worse in the one case than in the other.

On the contrary, the coal furnished and used on the 23d, 24th and 25th Oct., with the Return Tubular boilers, was screened, bright and noticeably clean and dry; and there was besides some rescreening during the night, but Mr. Peifer, engineer of the Brush Electric Light Company, says, "not much." One hundred pounds of the former, put on top of the boilers, lost 7.37 lbs. in drying, equal to 7.37 *per cent.* of surface water. One hundred pounds of the latter, similarly treated, lost only 2.87 lbs. in drying, equal to 2.87 *per cent.* at surface water, a difference of 2.57 to 1.*

The data, observations and information obtained by Mr. Hoadley and Mr. Le Van and their several assistants, and attested by the signature of both parties to each day's record, consisted of the following particulars:

(1.) Weight of water pumped into boilers.

(2.) Weight of coal and kindlings put into furnaces.

(3.) Weight of ashes and residue at end of test.

(4.) Temperature of feed water as it left the heater, recorded every fifteen minutes.

(5.) Steam pressure by standard test gauge, recorded every 15 minutes.

(6.) Temperature of products of combustion (flue gases), taken every 15 minutes by an air thermometer.

(7.) Indicator cards from both ends of each engine, taken every 30 minutes; a pair of Tabor or Crosby indicators being attached to each cylinder.

(8.) Speed of engines, taken with a speed-counter.

(9.) Occasional volumetric analysis of flue gases.*

(10.) Occasional pyrometric measurements of the temperature of the incandescent coal, by the platinum-water pyrometer.

(11.) Numerous determinations of the quality of the steam by the steam calorimeter.

(12.) Careful ascertainment by measurement and inquiry, of all particulars of dimensions and arrangement of the respective boilers.

(13.) Ascertainment of the number of arc-lights in use, supplied by the dynamo driven by each engine; obtained from the Secretary of the Brush Electric Light Company of Philadelphia.

Each and all of these observations, together with their aggregates and mean values have been compared and mutually agreed on as correct by Mr. Le Van and Mr. Hoadley through their assistants who kept the notes, respectively, and each page of transcript of such compared notes has been signed by both parties.

Upon these values, so verified, the calculations of the results which follow have been made.

Although a marked contrast in the

* On this subject of the extreme difference in the quality and condition of the coal used in the tests of the two sets of boilers reference is respectfully made to a joint statement of facts and joint protest drawn up at the time and signed by Mr. Le Van and Mr. Hoadley, Mr. Le Van confirming the allegations of said statement, by reference to his note-book, (not here given, but to be obtained on application to Mr. Hoadley.—J. C. H.

* Mr. Hoadley is alone responsible for the notes under (9) and (10).—J. C. H.

quality of the coal used with the two sets of boilers was apparent at a glance, and still more apparent on careful inspection; yet no allowance has been made on this account except for the ascertained difference in the quantity of surface water, as above explained.

The results of the determinations of entrained water in the steam, of the analysis of flue gases, and their temperature, and the temperature found in the heart of the fire, will be given below.

The height of barometer for each day, was obtained from the Chief of the Signal Service in Washington, as reported from Philadelphia.

Due allowance and correction have been made for all variations in the height of water in water-gauge glass and for all difference in steam pressure at beginning and end of each day's experiment.

Each pound of wood used for kindling has been reckoned equal to $\frac{4}{10}$ of a pound of merchantable coal, say with 10 *per cent.* of ash, and therefore equal to $\frac{3.6}{10.0}$ of a pound of combustible.

Each pound of cotton waste used for starting fires, has been considered equal to a pound of combustible.

EVAPORATIVE TEST: OCT. 18, 19, 20.

Babcock and Wilcox boilers: 21.5 *hours.*

16388.5	pounds of wet coal thrown on grate.
1207.8	pounds of surface water in coal.
15180.7	pounds of dry coal thrown on grate.
462	pounds of wood used for kindling.
72.5	pounds of cotton waste, for starting fires.
3305	pounds of ashes and residue.
11875·7	pounds of combustible (in coal) consumed.
166.3	pounds of combustible= 462 lbs. of wood.
72.5	pounds of combustible= cotton waste.
12114.5	pounds of combustible consumed.
134410015	* B. t. u. apparently received by boiler.
130176100	B. t. u. actually received —water allowed for.
10745.48	B. t. u. received per pound of combustible.
11.127	pounds of water evaporated from and at 212° F. per 1 lb. of combustible.
74.18	*per cent.*, apparent efficiency.
1497793	B. t. u. required to dry the coal.
0.128	pounds of water evaporated from and at 212° F. per 1 lb. of combustible, equal to drying the coal.
11.255	pounds of water evaporated from and at 212° F. per 1 lb. of combustible.
75.03	*per cent.* actual efficiency.

* British Thermal Units.

EVAPORATIVE TEST: OCT. 23, 24, 25.

Return Tubular Boilers: 16 *hours.*

13171.5	pounds of screened coal thrown on grate.
378	pounds of surface water in coal.
12793.5	pounds of dry coal thrown on grate.
319	pounds of wood used for kindling.
34.5	pounds of cotton waste, for starting fires.
2697	pounds of ashes and residue.
10096.5	pounds of combustible (in coal) consumed.
115	pounds of combustible= 319 lbs. of wood.
34.5	pounds of combustible= cotton waste.
10246	pounds of combustible consumed.
106300397	B. t. u. apparently received by boiler.
104110609	B. t. u. actually received. water allowed for.
10161.1	B. t. u. received per 1 lb. of combustible.
10.522	pounds of water evaporated from and at 212° F. per 1 lb. of combustible.
70.15	*per cent.* apparent efficiency.

482555 B. t. u. required to dry the coal.
0.049 pounds of water evaporated from and at 212° F. per 1 lb. of combustible expended in drying the coal.
10.571 pounds of water evaporated from and at 212° F. per 1 lb. of combustible.
70.47 *per cent.* actual efficiency.

COMPARATIVE ECONOMY BY THE EVAPORATIVE TEST.

$11.255-10.571=0.684$; and $\frac{.684}{10.57}=.0648$ $=6.47$ *per cent.*

ENGINE TEST: OCT. 18, 19, '20.

Babcock and Wilcox Boilers: 21.5 *hours.*

130.41 mean indicated horse-power.
21.5 hours, duration of experiments.
12114.5 pounds of combustible consumed.
563.46 pounds of combustible consumed per hour.
4.321 pounds of combustible consumed per h. p. per hour.
130156. pounds of water evaporated.
6054 pounds of water evaporated per hour.
46.57 pounds of water evaporated per h. p. per hour.
45.1 pounds of dry steam per h. p. per hour.
10.43 pounds leakage per h. p. per hour.
34.67 pounds of dry steam *used* per h. p. per hour.

ENGINE TEST: OCT. 23, 24, 25.

Return Tubular Boilers: 16 *hours.*

137.78 mean indicated horse-power.
16 hours, duration of experiments.
10246 pounds of combustible consumed.
640.375 pounds of combustible consumed per hour.
4.648 pounds of combustible consumed per h. p. per hour.
104562. pounds of water evaporated.
6535 pounds of water evaporated per hour.
47.43 pounds of water evaporated per h. p. per hour.
46.45 pounds of dry steam per h. p. per hour.
12.33 pounds leakage per h. p. per hour.
34.12 pounds of dry steam *used* per h. p. per hour.

COMPARATIVE ECONOMY BY THE ENGINE TEST.

$4.648-4.321=0.327$, and $\frac{.327}{4.321}=.0757=$ 7.57 *per cent.*

EXPLANATIONS OF THE LIGHT TEST.

(*See Table next page.*)

1. The engines were at all times run under the direction of the engineer of the Brush Electric Light Company. No interference was at any time attempted with the selection of the engines to be run, the number to be used, or the speed at which they were to run.

2. Engine No. 1 was run on Oct. 18, 19 and 20, at rather less than half its full speed, with the throttle-valve only very slightly opened, merely driving the shaft of the dynamo, without current, consuming a quantity of steam, small in itself, but disproportionate to the work done in overcoming friction, and by so much increasing the quantity of combustible per hour for each indicated horse-power, and in a more marked degree the quantity consumed per hour for each arc light. Since this engine consumed *some* steam and produced *no* light, the combustible per light was, so far as this engine was concerned, infinite. There were doubtless good reasons for running it in this manner, as already explained.

On Oct. 23, the first day of the test of the Return Tubular Boilers, this engine was indeed kept in motion, but at so low a speed, produced by extreme throttling, that its indicated power fell off from 1.31 H.P. to 0.54—only 41 *per cent.* as much. This change affected injuriously, but in a very small degree, the economy of the engine test, but favorably, and in a greater degree (though still only a little), the light test. On Oct. 24 engine No. 1 was made to develop nearly 27 H.P. in order to supply 11 regular lights and a test light, 12 in all, and No. 5 and started at a very low speed,—throttled—giving only 0.36 H.P., only 27 *per cent.* as much as

LIGHT TEST, OCTOBER 18, 19, 20,—23, 24, 25.

1882. Number of engine.		Babcock and Wilcox boilers.			Return Tubular boilers.		
		Oct. 18. H.P.	Oct. 19. H.P.	Oct. 20. H.P.	Oct. 23. H.P.	Oct. 24. H.P.	Oct. 25. H.P.
Indicated power H.P.	1	1.34	1.30	1.30	0.54	26.86	27.04
	2	43.05	43.60	40.89	39.95	37.73	38.55
	3	42.20	41.85	42.46	40.02	39.51	41.69
	4	44.16	45.57	43.29	41.83	42.19	42.58
		..	..	.	..	0.36	0.00
Sums.....		130.75	132.32	127.94	122.34	146.65	149.86
True totals.....		130.78	132.71	127.72	123.36	147.15	150.83
Number of 2000 candle lights.	1	0	0	0	0	12	12
	2	40	40	40	41	41	41
	3	40	40	40	41	41	41
	4	41	41	41	40	40	40
	5	0	0	0	0	0	0
Totals.....		121	121	121	122	134	134
Indicated power per arc light. H.P.	1	..	..	..	..	2.2383	2.2533
	2	1.0763	1.0900	1.0223	0 9744	0.9202	0.9402
	3	1.0550	1.0462	1.0615	0.9761	0.9637	1.0168
	4	1.0771	1 1115	1.0506	1.0458	1.0547	1.0645
Mean.....		1.0808	1.0968	1.0555	1.0111	1.0981	1.1331

Grand mean :	B. & W. Co.		Ret. Tub.
Power.....	130.41	..	137.78
Hours run.....	21.5	..	16.
Lights run.....	121.	..	128.75
H.P. per light.....	1.0703	..	1.0701
Combustible per light per hour..	4.6567	..	4.9738

COMPARATIVE ECONOMY BY THE LIGHT TEST.

$$4.9738-4.6567=.3171\,;\ \text{and}\ \frac{.3171}{4.6567}=.0681=6.81\ \text{per cent.}$$

was developed by No. 1 on Oct. 18, 19 and 20. Further slight diminution of engine economy, and increase of light economy. On Oct. 25, the last day's test of the Return Tubular Boilers, this spare engine disappears altogether. Although it continued running at a very low speed, the diagrams were a mere line, and were discontinued.

3. On Oct. 23, the total power falls off to 123.36 I.H.P. from a mean of 130.41 I.H.P. for the first 3 days, a reduction of 5.41 *per cent.*, largely due to a reduction of speed of engines No. 2, 3 and 4. The effect of this reduction of speed on the power developed was direct and apparent. Its effect on the *lights*, merely a reduction of their intensity and whiteness, may or may not have been perceived, commented on, or complained of. Their actual illuminating power was certainly a little reduced by this lowering of speed. Of the motives for this change of speed, we know nothing, and speak only of its obvious and necessary effects. The most obvious effect was to reduce the mean power of engines No. 2, 3 and 4, per light, for Oct. 23, 24 and 25, to a little under 1 indicated H.P. (0.9952) a reduction from the mean of Oct 18, 19, 20, (1.0803 I.H.P) of 7.92 *per cent.* But a sudden demand for extra lights during the Penn Bicentennial, made it necessary to start up another dynamo, although with only 11 lights for illumination and 1 test light making 12, just 30 *per cent.* of 40 lights, yet actually

causing the development of almost 27 horse power (26.95 I.H.P. mean of Oct. 24, 25, engine No. 1), which is 62 *per cent.* of the full power of engines No. 2, 3 and 4 on Oct. 18, 19 and 20, when producing currents to supply 40⅓ lights each at the higher speed. The inevitable result was to augment the power per light for engine No. 1 on Oct. 24 and 25, in the compound ratio of these two numbers: $.62 \div .3 = 2.067$ and $1.0803 \times 2.067 = 2.2329$, which is substantially the power per light for this engine (2.2383, and 2.2533). Low as are the several quantities of power per light for engines No. 2, 3 and 4, during the last three days, yet, adding in No. 1 brings up the mean to almost exactly the same as on the 3 first days (1.0703 and 1.0701 H.P.) — although the *light* was certainly less. It should be mentioned that the speed of No. 1 was not altered; it ran, when producing light, at the speed at which Nos. 2, 3 and 4 ran on the first 3 days.

4. It will be noticed that the total power appears, in the light test, in two lines, with slight differences. The first line gives the sums of the figures above, as they stand. The lower line gives the geometrical or true mean of the horse powers for each days run.

5. The result of the light test, is to be found in the relations to each other at the final figures. The Babcock & Wilcox boilers consumed 4.6567 pounds of combustible per hour for each light burning. The Return Tubular boilers consumed 4.9738 pounds of combustible per hour for each light burning. The difference, $4.9738 - 4.6567 = .3171$, and .3171 divided by 4.6567 gives as a quotient .0681, equal 6.81 *per cent.*, which is the loss of the Return Tubular boilers as compared with the Babcock& Wilcox boilers by the light test.

Having now applied three tests, namely: (1) water evaporated, (2) power developed, and (3) light produced, all per 1 pound of combustible consumed, it remains to apply the fourth, by comparing the heat carried off by the gases of combustion escaping to the chimney from the two sets of boilers respectively. The flue gases gave, by volumetric analysis, the following parts in 100 of dioxide of carbon (carbonic acid), resulting from the complete combustion of carbon.

CARBON DIOXIDE IN FLUE GASES.

Babcock & Wilcox Boilers.			Return Tubular Boilers.		
Day in October, 1882.	By Volume. Parts in 100.	By Weight. Parts in 100.	Day in October, 1882.	By Volume. Parts in 100.	By Weight. Parts in 100.
18	7.20	10.65	23	7.00	10.36
19	8.00	11.84	24	7.70	11.40
20	7.80	11.54	25	7.30	10.80
Mean,	7.67	11.34	Mean,	7.33	10.85

Flue gases per 1 lb. of combustible $= \frac{95}{11.34 \times \frac{3}{11}} = 30.71$.

Mean temperature of flue gases, observed, 467° F.

$30.71 \times .238 = 7.309$ lbs. water. Equivalent evaporation at and from 212° F. $= \frac{7.309 \times (467 - 60)}{965.7} = 3.081$ lbs.

$\frac{3.081}{15} = 20.54$ per cent. *loss.*

Flue gases per 1 lb. of combustible $= \frac{95}{10.85 \times \frac{3}{11}} = 32.10$.

Mean temperature of flue gases, observed, 543° F.

$32.10 \times .238 = 7.64$ lbs. water. Equivalent evaporation at and from 213° F. $= \frac{7.64 \times (543 - 60)}{965.7} = 3.821$ lbs.

$\frac{3.821}{15} = 25.47$ per cent. *loss.*

15 lbs. of water evaporated from and at 212° F., being the full evaporative power of 1 lb. of our combustible.

LOSSES OF HEAT AND EFFICIENCY.

	Babcock & Wilcox Boilers. Parts in 100.	Return Tubular Bolers. Parts in 100.
Loss of heat, carried off by heat gases	20.54	25.47
Loss by imperfect combustion, and radiation	4.43	4.06
Aggregate losses	24.97	29.53
Actual efficiency	75.03	70.47
Total heating power of combustible	100.00	100.00

Loss carried off by hot gases, Ret. Tub. boilers 25.47%

Loss carried off by hot gases, B. & W. boilers 20.54%

Difference; greater loss by Ret. Tub. boilers 4.93

This difference, or excess of heat lost by the Ret. Tub. boilers, divided by the efficiency of these boilers (70.47 *per cent.*), gives the ratio of the excess of loss to actual efficiency:

$$\frac{4.93}{70.47} = .06996 = 7.00 \textit{ per cent.}$$

SUMMARY OF RESULTS BY THE FOUR METHODS.

	Babcock & Wilcox Boilers.	Return Tubular Boilers.	Difference in favor of B. & W. Boilers.	Difference *per centum.*
Evaporative test.....	11.254	10.570	.684	6.47
Power, engine test..	4.321	4.648	.327	7.57
Light test.....	4.6567	4.9738	.3171	6.81
Test by loss at chimney.....	20.54	25.44	4.9	7.00
Mean of four tests..				6.96

Explanation of Table. — The Babcock & Wilcox boilers evaporated *more* water for each pound of combustible consumed; consumed *less* combustible per hour for each indicated horse power produced; consumed *less* combustible per hour for each arc light in use; and lost *less* heat by hot gases escaping to the chimney, than the Return Tubular boilers. The smallest ratio of difference is 6.47 *per cent.*; the largest ratio of difference is 7.57 *per cent.*; and the mean ratio of difference in favor of the Babcock & Wilcox boilers, is 6.96 *per cent.*

While doing this, they were evaporating 6054 pounds of water per hour, into steam, containing only 3.15 *per cent.* of entrained water, leaving 5863 pounds of dry steam per hour, enough at the rate of 30 pounds of dry steam per hour for each horse power, to supply 195 horse power which is 30 *per cent.* above their rated power.

This comparison leaves out of view all disparity of coal save the ascertained difference in surface water.

TEMPERATURE OF THE GASES OF COMBUSTION.

Without actual analysis of the coal used, it is impossible to say just what was the composition of the "combustible," so called, meaning thereby all the coal and the coal-value of kindlings thrown on the fire-grates, less the surface-water and the ashes and residue left at the close of each experiment. It is certain that "combustible" so ascertained, included (1) some water held in the coal in a manner not permitting it to be dried away, as surface water, and (2) some ash carried along with the escaping gases, and deposited in the flues and chimney. These are small in amount, and not being accurately ascertainable are not here taken into account. There was also, certainly a small quantity of sulphur, oxygen and nitrogen, not to be separately discriminated, amounting in the aggregate to about 2 *per cent.*; and a small quantity, probably $1\frac{3}{4}$ to $1\frac{1}{2}$ *per cent.* of hydrogen, perhaps combined in some form with carbon, but of some value as a fuel. Basing my judgment on numerous analyses of coal in other cases, I estimate the full heating power of the "combustible" to be equal to that of carbon, 14500. British thermal units equal to the evaporation of 15 pounds of water at 212° F. into steam of the same temperature under the pressure of one atmosphere; and that 95 *per cent.* of this combustible was carbon, and 1.15 per cent. free hydrogen. I also estimate that there was also a loss of 1 *per cent.* in consequence of imperfect combustion, resulting from the presence in the flue gas of a very small trace of carbon monoxide, (CO).

We have then full heating power of 1 lb. of "combustible," B. t. u.	14500.
Less for imperfect combustion, 1 *per cent*.....	145.
Heat actually produced per 1 lb. of combustible.....	14355.
*Water in coal actually ascertained, 2.87 *per cent*.....=	.0287
Water formed by combustion of hydrogen in coal, 9×.0115...=	.1035
Water in coal, and formed by combustion of hydrogen.....	.1322
B. t. u. in 152° F. (60° to 212° F.)=	152.9
From this we must subtract the product of 152° by .48 (=specific heat of steam), as it appears in another place, later, and 152. ×.48.....=	72.9
	80.

* Test of Ret. Tub. boiler, Oct. 25. All relating to the temperature of the coal in the furnace, applies solely to this gate.—J. C. H.

and .1322 × 80. = 10.5760, which subtracted from the above....14355
10.6

Leaves available heat per 1 lb. combustible 14344.4

Now we have 32.10 pounds of flue gases per pound of combustible, the specific heat of which gases is .238, and

$$32.10 \times .238 = 7.6398.$$

This is the value in pounds of water of the heat-capacity of the flue gases formed by the combustion of 1 pound of "combustible."

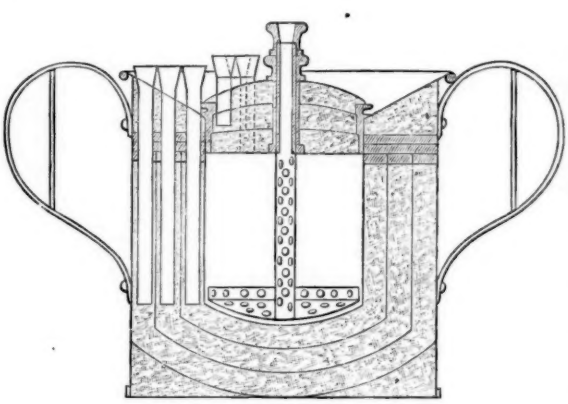
But the total number of available heat units, (B. t. u.) from 1 pound of combustible, is 14344.4; therefore the resulting *mean* temperature will be

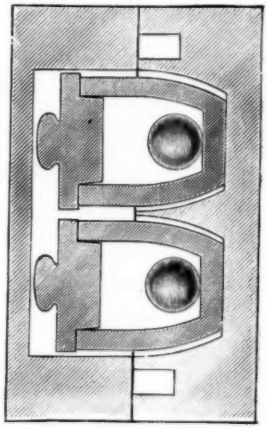
$$\frac{14344.4}{7.85278} + 60 = 1886^\circ \text{ F.}$$

60° being the initial temperature of the the air.

The temperature, however, after the fire was well ignited, the water driven off, and the hydrogen consumed, would be higher, as follows:

$$\frac{14355}{7.64} + 60 = 1939^\circ \text{ F.}$$





The air may be estimated to have contained on entering the furnace, 1 *per cent.* of moisture, and (32.10−.95=31.15)×.01..... 0.3115

Bringing forward the water in the coal and formed by the combustion of hydrogen, (above.)..... 0.1322

Total moisture per 1 lb. of combustible..... 0.4437

This multiplied by .48=specific heat of steam, we have

.4337×.48= .21298

To which add the water value of the heat-capacity of the flue gases per 1 lb. of coal brought forward.....7.63980

Total value, in the equivalent weight (pounds) of water, of all the heat absorbents in the flue-gases per 1 lb. of combustible. 7.85278

The temperature actually found in the fire, by the platinum-water pyrometer; a crucible * containing the platinum heat-carrier being buried in the hottest part of ignited coal, was by two experiments

2281°
2259.5°

Mean.....2270.° F.

This would show that in the part of the fire where this temperature was taken, the quantity of flue gases per pound of combustible was

$$\frac{14355}{(2270-60)\times .238} = 27.29.$$

The difference between this quantity, and the quantity actually found in the flue, 32.10 is 4.81 and $\frac{4.81}{27.29} = .176 =$ say nearly 18 *per cent.*

* The fire brick shown in cut were not used: the crucible was put naked into the fire.—J. C. H.

It is not difficult to account for this. Some air always finds its way into the flue or chimney, which does not pass *through* the incandescent coal; by open doors, while firing, or cleaning fires, by leakage at fire-doors and arch-fronts, and by infiltration through brick-work—the latter quantity being much more than is commonly supposed. In this case, there was a special reason in the mode of firing, for a larger quantity of air per square foot passing through the *whole* surface of the fire, than through that portion of it where the temperature was taken. That reason was that the coal, when thrown on the grates, was not spread evenly, so as to make a fire of uniform depth, but was heaped up at a little distance from the fire-doors, so as to be noticeably thicker at that place than from there to the bridge wall. Now it was precisely in the thickest part of the fire, where the air was most obstructed, and entered through the coal in smallest quantity, that the crucible containing the platinum ball was placed, resulting necessarily in showing a temperature at that point higher than the mean temperature, and a quantity of air smaller than the mean quantity. At this point the water was all driven off, and the hydrogen all consumed. The temperature found, .2270° F., is to be compared with that of a fire in corresponding condition, but with 32.10 pounds of flue gases per pound of combustible, which was 1939° F. The difference=331° F., divided by (1939−60) =.176, the same as before.

Comparing the temperature produced by a well ignited fire, 1939° F. with the corresponding observed temperature of flue gases:

$$\frac{543-60}{1939-60}=.2570=25.70 \textit{ per cent.}$$

Again; comparing the temperature found in the heart of the fire at its thickest (and hottest) part, with the observed temperature of the escaping flue gases:

$$\frac{543-60}{2270-60}=.2185=21.85 \textit{ per cent.}$$

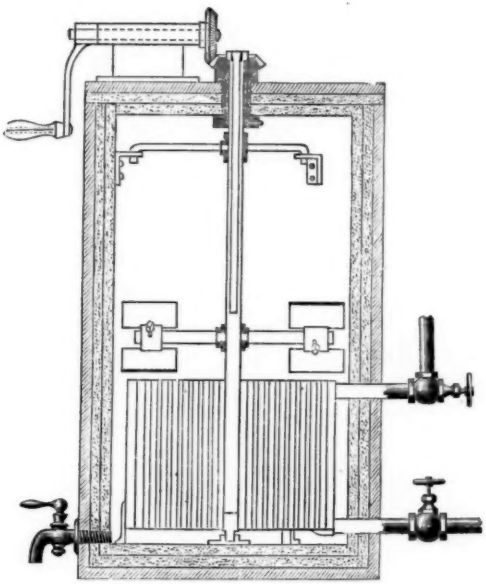
The actual loss was 25.47 *per cent.* The first, which represents mean conditions, agrees substantially with the last; the difference, 25.70−25.47=0.23, being insignificant. The second, which represents conditions far above the mean, yet agrees so well with the last as to be strongly confirmatory of it. The difference, 25.47−21.85=3.62=14 *per cent.*, is no more than must be expected from the disparity of conditions. In effect, the heat generated in the thickest part of the fire was diluted by air passing in more rapidly through the thinner parts of the fire, and leaking in about doors and arch fronts, and elsewhere, and drawn in through the occasionally opened fire door, so as to reduce the temperature of the gases by augmenting their mass; and consequently the loss was carried up from 21.85 *per cent.* which it would have been with no more air than actually passed *through* the thickest part of the fire, to 25.47 *per cent.*, as it was found to be with the quantity of gases actually present in the flue leading to the chimney.

Calorimetric Experiments.

The condition of the steam as to entrained water, was ascertained by means of Mr. Hoadley's steam calorimeter. This instrument had been very successfully used in precious work, and much care had been taken in its construction and in ascertaining its own proper heat-capacity.

Briefly described, this calorimeter consists of a cylindrical vessel of 24 oz. tinned copper encased in a jacket of heavy galvanized iron, the inner and outer vessels being concentric cylinders, leaving a space 2.25 inches thick all around and a similar space at the bottom, which is filled with two 0.75 inch layers of eider-down surrounded by 0.75 inch of hair felt. A cover of similar construction, thickness and filling, shuts in to the depth of 0.75 in., the remaining 1.5 in. extending out flush with the outside of the case.

The vessel holds rather over 200 pounds of water, and is usually charged, for an experiment, with about that quantity. An agitator, in form a two two-bladed propellor, is turned by a crank which does not interfere with the use of a thermometer placed deep down in the hollow shaft. The heat-capacity, carefully determined by three independent methods, all admitting of much accuracy, and differing but little in the results of many experiments, is equal to that of 17.2 pounds of water. The thermometer used to



measure the increase of temperature is 32 inches long, with a long bulb, and a safe-bulb a-top, graduated to degrees and tenths of a degree F., and has a range of 48° (32° to 80° F.), in about 28 inches. This gives nearly 0.6 in. to 1° and admits of accurate reading to $\frac{1}{100}$ degree. Steam was condensed in a tubular copper drum forming a surface condenser, from which the water resulting from the condensation of steam together with the entrained water was drawn off and weighed on a delicate balance. The data of one of the nine experiments with steam drawn from the Babcock and Wilcox boilers, will be found below.

Time of Readings.	Weight of calorimeter and contained water.		Weight of water in condenser.		Temperature of water.		Difference: Increase of temperature.	Pressure by steam gauge above atmosphere.
	Before admitting steam.	After admitting steam.	By difference.	By weighing after drawing off.	Before admitting steam.	After admitting steam.		
h. m.	lbs.	lbs.	lbs.	lbs.	deg. F.	deg. F.	deg. F.	Pounds per sq. in.
1	2	3	4	5	6	7	8	9
..	529.	535.25	6.25	6.305	..	..	..	101.7
12.51	..	..	..	..	44.88	..	..	..
12.52	..	..	..	..	44.92	..	..	..
12.53	..	..	..	..	44.96	..	..	..
12.53½	Steam admitted.			..	44.98	77.68	32.7	..
12.54	Steam shut off.			..	..	..	..	..
12.55	..	..	..	..	..	..	..	..
12.56	..	..	..	..	..	77.68	..	..
12.57	..	..	..	..	..	77.68	..	..

Total heat above 0° F. in steam of 101.7+14.7=116.4 lbs. pressure absolute 1217.25

Heat in British thermal units imparted to water per 1 lb. of steam condensed:

$529.0+17.2-332.0=214.2$;

$$\frac{214.2\times 32.73}{6.305} \quad . \quad . \quad . \quad 1111.94$$

Heat in B. t. u. that 1 lb. of steam would give up in condensing, if saturated: $1217.25-77.71 = 1139.54$

Heat in B. t. u. that 1 lb. of water at t. corresponding to 116.4 lbs. per sq. in abs. would give up in cooling to 77.68° F.=342.43−77.71 . . . . . = 264.72

Then,

$$\frac{1139.54-1111.94}{1139.54-264.72}=\frac{27.6}{874.82}=.0315=3.15$$

per cent.

The results of these calorimetric tests are subjoined:—

Babcock & Wilcox Company's boilers.		Return Tubular boilers.	
Date. 1882. Oct.	*Per cent.* water.	Date. 1882. Oct.	*Per cent.* water.
18	4.29	23	1.72
18	3.76	23	3.21
18	3.19	24	1.24
19	3.03	25	1.98
19	2.80	25	2.13
19	1.92		
20	2.89		
20	3.36		
20	3.15		
Mean.....	*3.15	Mean.....	*2.06

* Mr. Nystrom, by another apparatus, and by a method, correct in principle, but hardly admitting of the degree of accuracy attainable by the method described in the text, obtained the following results:

October 19, 1.8 *per cent.*
20, 1.4 "
21, 0.91 *per cent.*
25, 1.1 "

The mean of the two experiments with the B. & W. boilers, Oct. 19, 20, is 1.6 *per cent.*

The observation for Oct. 21, was not made while the tests were going on; but being under the same conditions, there is no good objection to taking it in. Then the mean of 0.91 and 1.1 is, 1.05 *per cent.* The ratio of these numbers to each other is the same as that of the means of the table in the test: Thus,

$$\frac{1.05}{1.6}=.656 \text{ and}$$

$$\frac{2.06}{3.15}=.654.$$

Experience with other return tubular boilers, at a lower rate of evaporation, leads me to think that the entrained water was, in both these sets of boilers, higher than is given by Mr. Nystrom's experiments, and I have preferred to insert my own determinations in the text, rather than the more favorable determinations of Mr. Nystrom. The largest result, 3.15 *per cent*, is not excessive, and when the heavy demand made upon these boilers for steam to supply the leakage of the engine valves is taken into consideration, the quantity of entrained water appears moderate.—J. C. H.

Condensation in Steam Pipes.

That portion of the steam which condenses in the steam pipes, and of the water carried over from the boilers, which settles and collects in the bottom of the pipes, is allowed to drain off through a drip pipe a quarter of an inch in diameter, and the flow is so regulated by a stop-cock as to prevent at once an escape of steam and an undue accumulation of water.

An experiment was made to determine the quantity so carried off. A bucket was filled with ice-water, weighed and set to receive the water from the drip-pipe conducted to it by a flexible tube. At the end of five minutes it was again weighed, and found to have gained 4 lbs. 3 oz. 1 drm. avoirdupois, = 4.1914 lbs. Twelve times this quantity = 50.3 lbs. would be discharged in an hour, and as the evaporation was at the rate of 6054 lbs. per hour the ratio of the drip to total evaporation $=\frac{50.3}{6054}=.0083$, is 0.83 *per cent.*

It follows that the quantity of water carried over by the steam and formed by condensation in the steam pipes, is diminished by this quantity of drip, which is equal to $\frac{83}{100}$ of one *per cent.* of the total quantity evaporated.

Surface Disturbance.

The mean steam pressure during the test of the Babcock & Wilcox boilers, October 18, 19 and 20, was 115.1 lbs. per square inch absolute.

The weight of 1 cubic foot of steam of this pressure is 0.264256 lb.

The weight of water evaporated per hour was 6054 lbs., and therefore the volume of steam generated per hour was $\frac{6054.}{.264256}=22909$ c. ft.$=6.363$ c. ft. per second. The area of water surface for the disengagement of steam in these boilers, being 80 sq. ft., the quantity of steam generated per second, per sq. ft. of water surface is $\frac{6.363}{80}=.07954$ c. ft.

This is almost 1 c. in. (0.954 c. in.) per second from every square inch and equal to 0 004 in evaporated per second from the water, lowering its surface 0.24 in. per minute.

For the Return Tubular boilers, the mean steam pressure was, during the test, Oct. 23, 24 and 25—119.8 lbs. per sq. in. absolute, and the weight of 1 c. ft. of steam of this pressure is 0.273862 lb. The water evaporated per hour was 6535 lbs. and it follows that the volume of steam generated per hour was $\frac{6535}{.273862}$ =23862 c. ft. =6.6283 c. ft. per second. The water-surface for these three boilers being 130.2 sq. ft., the quantity of steam generated per second per sq. ft. of surface is $\frac{6.6283}{130.2}$ =.05091.

This is equal to 0.6309 c. in. from each sq. in. per second, and involves the evaporation of 0.00277 in., depth of water per second, lowering the surface nearly 0.17 in per minute.

We are thus afforded a means of comparing the violence of the surface agitation in the two cases, or the upward thrust of the steam bubbles, and their tendeney to carry along, in mechanical suspension, particles of *entrained water*.

The actual proportion of entrained water as found by calorimetric experiments was, for the Babcock & Wilcox boilers, 3.15 *per cent.*, and for the Return Tubular boilers, 2.06 *per cent.*, and the ratio of the latter to the former $\left(\frac{2.06}{3.15}\right)$ is as 1 to 1.53.

On the other hand, the ratio of the disturbance, that is, of the respective volumes of steam generated each second from each 1 square unit of water surface is $\left(\frac{.07954}{.05091}\right)$, as 1. to 1.567, a result substantially the same as before, the difference being less than 2 *per cent.* The two methods are therefore mutually confirmatory.

Conclusion.

The general result—a difference of about 7 *per cent.*, in favor of the Babcock & Wilcox boilers, arrived at by four independent methods of comparison, all free from objection, and together, mutually confirmatory in the highest degree, is what might have been confidently predicted upon a comparison of the respective proportions of heating-surface to fire-grate area, and other data, of the two sets of boilers.

REPORTS OF ENGINEERING SOCIETIES.

Engineers' Club of Philadelphia.—At the meeting of Nov. 4th, Mr. John Haag read a paper on the number and tonnage of British ships built in 1881.

Mr. Charles Darrach read a paper on the Pollution of Water.

At the meeting of Nov. 18th, Mr. Charles W. Pusey presented a paper upon the Twin Screw Steamer "Victoria." On Tuesday, Nov. 7th, this steamer sailed from Wilmington, Del., for Greytown, Nicaragua. This vessel is a light draught twin screw steamer for service on Lake Nicaragua, and of a class that is attracting some attention from those interested in the economical transportation of freight on bays and rivers, where the draught of water is limited, and where the side wheel steamer is principally used.

The hull is of iron and is 136′ 6″ length over all, 26′ beam and 7′ deep above cross floors. The model is the same as that of several side-wheel steamers built for service on rivers and bays in South America and Mexico. She has one fore and aft bulkhead in center, and four athwartship, all made water-tight. The compartment aft is fitted for water ballast to trim the vessel. The frame is of angle iron. The machinery consists of two compound engines with cylinders 13″ and 21″ diam. x 18″ stroke, fitted with jet condensers. The engines are independent, each driving a propeller wheel 6′ diameter. She has two steel boilers of the locomotive type, fitted for burning wood and constructed for a working pressure of 100 lbs. per sq. inch. The finished draught of water with 5 tons of coal in bunker, was 4′ 6″ aft, and 3′ 6″ forward. On trial trip with a draught of 5′ 4″ aft and 2′ 10″ forward, she made a speed of 10 knots per hour, with 119 revolutions per minute, 94 lbs. 26″ vacuum; total indicated horse power 246. During the trial the ballast tank was filled with water.

When she sailed for Greytown she had a cargo of 105 tons of coal, also merchandise and stoves amounting to about 20 tons more, the draught of water being 6′ 3″ aft and 5′ forward. Under these conditions going down the bay, she made 9½ knots per hour, with 80 lbs. steam pressure and 108 revolutions per minute.

Prof. L. M. Haupt presented a profusely illustrated description, which he has been compiling for some time, of the progress of early American Wooden Bridge Building. Reference was made to the well-known structures of Schaffhausen, 1758, and Wettingen, both destroyed by the French in 1799, and to the remarkable bridges by Wernwag, between 1804 and 1812. A summary of the typical forms as

represented in the Burr, Town, Haupt, the Longs, Howe & Pratt trusses, was then given with illustrations and reference to the works of Mr. Chevallier, Lavoinne, Pontzen and others.

Attention was called with regret to the fact that the best illustration and data with reference to the engineering works of America were generally only obtainable from foreign sources.

The paper gave rise to a general discussion, in which Messrs. Cleemann and Graff cited instances of early suspension bridges (Mr. Graff presenting the Club with a photograph of a painting, showing the old suspension bridge at the Falls of Schuylkill, Philadelphia), and reference was made to the early floating draw constructed by the late Henry R. Campbell, at Rouse's Point; also to a wooden truss said to have been built about 1809, over the Terrebonne River, near Montreal, Canada, of *six hundred* feet span, but of which no very authentic data could be obtained.

A communication from Mr. M. Coryell, was received through Mr. Wm. A. Ingham, describing the early structure built by Wernwag, at New Hope, over the Delaware, with illustrations of several of the Wernwag bridges. The description was in the handwriting of Mr. Ingham's father, then president of the bridge company.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—At a Regular meeting, held on Dec. 6, Vice-President Paine in the chair, the following were elected members: Timothy Cookson Bradley, Assistant U. S. Engineer, St. Joseph, Mo.; Henry J. Gielow, Assistant U. S. Engineer, Town Creek, Ala.; Edward S. Safford, Division Engineer, New York, West Shore & Buffalo, and New York, Ontario & Western Railroads, Newburgh, N. Y.; James Dix Schuyler, Chief Engineer and General Superintendent, Sinaloa & Durango Railroad, Culiacen, Sinaloa, Mexico; Thomas Wellington Spencer, Civil Engineer, Utica, N. Y.; Russell Thayer, Chief Engineer and Superintendent Fairmount Park, Philadelphia, Pa; Edmund Brownell Weston, Engineer in charge Water Department, Providence, R. I.

Mr. J. J. R. Croes, M. Am. Soc. C. E., described the progress that had been made in the procuring of supplies of water for cities and towns from subterranean sources, or "ground water," describing and illustrating the galleries, basins and wells which have been constructed in the United States within the last 13 years. The theory on which the first works of the kind were constructed was that a supply could be procured by filtration through the gravel of a bank of a river or pond. It was found, however, that more water came from the land side than the river, and that it was of a different character from the river water. It is now established that such supplies are furnished almost exclusively from the great underground streams or reservoirs, which are found in all the valleys of streams which are filled to a considerable depth with gravel. The failures and successes of many works were described, and the yield of filtering galleries discussed. The relative advantages of open and closed galleries were discussed, and it was shown that the experience of most places was decidedly against the advisability of open galleries or canals, on account of the growth of vegetation in them, although the water came from the gravel perfectly pure and cold, like spring water. An interesting discussion followed, in which Gen. George S. Greene, Mr. Joseph P. Davis, Mr. F. H. Leating, City Engineer of Halifax, and others participated.

ENGINEERING NOTES.

A DANUBE-ELBE CANAL.—The idea of joining the Elbe to the Danube by a waterway is a very old one, the construction of such a canal having been proposed as early as the reigns of Charles IV., Ferdinand II., and Joseph I. of Austria. In our own time the investigations connected with the subject of a canal from Linz, on the Danube, to Budweis, in Bohemia, have assumed practical shape in the form of a railway connecting the two towns. In a pamphlet published by the Vienna Danube Society, the idea has been revived. It is therein proposed to construct a canal which leaves the Danube at Kornneuburg, ascends in three main series of sluices, which together would overcome a rise of 1239 feet by means of 129 locks, to a plateau 45 miles long, whence it descends 538 feet by 56 locks to Budweis, where it joins the Moldau, a tributary of the Elbe. The whole length of the artificial canal would be 138 miles. The canalization of the Moldau from Budweis to Melnik, for a distance of 151 miles, would be effected by 62 locks, which together overcome a fall of 736 feet. The average distance of the locks from each other in the river would be 2½ miles; in the artificial canal, exclusive of the horizontal portion, ½ mile. The water supply of the canal on the plateau is to be provided for by the construction of reservoirs on the Upper Moldau and the Leinsitz, with corresponding feeders. The depth of water in the canal is to be 6½ feet; the width of the canal bed, 48 feet; the sluices have a width of 25 feet and a length of 208 feet. The sluices of the canalized river are about double as wide and long, and are consequently capacious enough to hold four vessels 192 feet long and 22½ feet beam at the same time. The water is to be dammed back by movable Poirée weirs combined with fixed overflowing weirs parallel to the direction of the current. The total cost of the canal and the canalization of the Moldau is estimated at £5,800,000, or about £20,000 per mile of waterway.

ANTWERP WATERWORKS.—The water for the supply of the town of Antwerp is drawn from the river Nèthe, at the Waelhem bridge, 10½ miles south of the town. The water, after having been purified, is sent in pipes, under a pressure of five atmospheres, from Waelhem to Antwerp.

The process of purification consists: 1st, in allowing the water as it comes from the river to settle the solid matter, and then decanting the clean water; 2d. Filtration through spongy iron and sand; 3d. A second filtration through

sand; 4th. The discharge of the purified water into the conduit.

The deposit from the water takes place in two basins alternately. These are filled three hours after each high-tide, when the water is in the best condition, by a 4-feet pipe from the river, passing through the embankment or dyke. Each basin holds 6,000 cubic meters (1, 320,000 gallons), and is filled in three-quarters of an hour. The water remains in each basin for twelve hours, and is then run off from the surface into pits, whence it is lifted by means of screw-pumps to the level required for delivering it into the filters.

There are two elevating screws, 40 feet long, and 3 feet in diameter, placed at an angle of 30°, driven each by a 12-H.P-engine. Each screw delivers 9 cubic feet of water per revolution, and they have eighty per cent. of efficiency. There are three filter-beds of spongy iron, placed side by side. The spongy-iron appears to be a bluish-gray scoria, of which the pores are much subdivided; it is in pieces of the size of hazel-nuts, prepared from a rich iron ore. It holds in its pores a large quantity of air minutely divided, in presence of the oxygen of which the organic matters in the water are burned or oxidized, when the water passes away fresh. Each filter-bed consists of three layers. At the bottom, the first layer, 3 feet deep, is a mixture of three parts of spongy iron and one part of pea gravel; then a layer of fine gravel about 4 inches deep; and, at the top, a layer of sand from Dordrecht, 2 feet thick. The foundation consists of beds of brick, open laid, through which the filtered water passes into a collecting channel, by which it runs off to the sand filters.

The sand filters are three in number, one to each filter of spongy iron, formed of three layers of sand and gravel. They detain pieces of iron oxide that escape from the iron filter, with the last impurities in suspension. The water, now fresh and pure, flows off into the reservoirs, which are circular and covered, and are constructed of cast-iron plates.

The pumping engines for forcing the water through the mains were constructed by Messrs. Eastons and Anderson. They are in two pairs, compound, of 100 H.P. each. The cylinders are 18 inches and 29½ inches in diameter, with strokes of 44 inches and 66 inches respectively, making, at maximum speed, twenty-two turns per minute. The pump delivers 48 gallons per revolution. of the engine.—*From Abstracts of Inst. Civil Engineers.*

RAILWAY NOTES.

NEW PROJECTS FOR A SIMPLON RAILWAY.—Even during the progress of the works connected with the now completed St. Gothard Railway, people in France began to think of means for meeting the competition that would be caused to French through traffic. Amongst the various proposals, the establishment of new railway communication with Italy by way either of the Simplon, Mont Blanc, or Little St. Bernhard, probably found most favor. The engineer's report concludes: "Two lines have principally engaged the attention of the committee, that over the Simplon and that over Mont Blanc. The one reaches the Italian frontier only after using Swiss railways for a length of 127 miles; the other leads direct from France to Italy. For the first line the tunnelling of the Alps would take place outside of France; the tunnel of the second line would have its northern mouth on French territory. The route over Mont Blanc would preserve to France her own traffic and to the port of Marseilles the through traffic of Switzerland; the Simplon line would, on the contrary, take our travelers and our goods over Swiss railways, and at the same time withdraw from Marseilles the through traffic above mentioned, handing it over to the port of Genoa and the Italian railways. Under these conditions, the majority of the committee have pronounced in favor of the Mont Blanc route, and recommend the adoption of the following resolution: The chamber requests the government to cause the project for a new international railway communication through the Alps, by way of Mont Blanc, to be submitted to an early and thorough examination."

We are unaware how far the investigations urged upon the French government with regard to the Mont Blanc line have been carried; but the surveys for the various routes of the Simplon Railway, which do not present the same engineering difficulties, have been actively pushed forward, as we learn from an article in the *Eisenbahn*. Both lines extend like feelers into the high Alps of the Swiss-Italian frontier. One coming from Geneva and Lausanne, ascends the valley of the Upper Rhone, and ends at Brieg; the other branches off at Novara, and extends as far as Gozzano and Arona respectively, according as they join on to the one or the other of the branches. The connecting line between the two railway systems requires a railway of a length of rather more than 60 miles, of which, however, about 12½ miles, or over 3 miles more than the St. Gothard tunnel, would form the great Simplon tunnel.

The direction of the projected Simplon tunnel is from northwest to southeast. The northern opening is about 1½ mile up the Rhone from the terminus at Brieg, at an altitude of 2205 feet, the southern tunnel end is placed below the village of Isella, at an elevation of 2008 feet, and 26 feet above the high-water level of the Diveria. Compared with the highest elevation of the St. Gothard tunnel (3693 feet), the Mont Cenis tunnel (4043 feet), and the Arlberg tunnel (4102 feet), that of the Simplon tunnel would be comparatively low, which would facilitate the construction of the approach railways, but, on the other hand, greatly increase the difficulties of constructing the tunnel on account of its great depth under the superincumbent mountain mass. The experiences gained in building the Mont Cenis tunnel, and still more the St. Gothard tunnel, show that one of the greatest difficulties in Alpine tunnel construction of great length and depth is the high temperature met with, which requires a perfect system of ventilation in order to make work at all possible. In the St Gothard tunnel the temperature is about 31 deg. C.; but it is well known that the means for ventilating it are defective, and

open to improvement. For a length of from 1300 to 1600 feet, the overlying mountain mass of the projected Simplon tunnel has a thickness of nearly 7000 (6944) feet, and, compared with what we find in the case of the St. Gothard, the maximum temperature in the interior of the Simplon tunnel would be between 32 deg. and 38 deg.

Professor Heim, of Zurich, has also made calculations with regard to the temperature in the interior of the projected Mont Blanc tunnel. The latter would lead from Chamounix to Pré-Saint Didier; its elevation would be 3434 feet above the sea, and its length about 12 miles. The result of his investigations was that, with an overlying strata here present of 11,200 feet, the temperature would be 40 deg. C. for a length of the tunnel of 3¼ miles, and for a length of 2.2 miles even 55 deg. C., without the possibility of lowering the temperature by a change of the level from the straight, as in the case of the Simplon tunnel. But the formation of the soil permits of the portion of the tunnel under Saint-Didier to be ventilated for a length of about 3 miles by shafts of a depth of not more than 640 feet.

As regards the cost of the rival railways, the only available estimates are those furnished by the committee of the French chamber. According to them, the construction of the Simplon Railway (Brieg, Isella, Pré di Mulera, Arona) would involve an expenditure of about 145,000,000 francs (£5,800,000); that of the Mont Blanc Railway (Annemasse, Chamounix, Pré-Saint Didier, Aosta-Ivrea together about 120 miles) would cost 179,000,000 francs (£7,160,000). The expense for the tunnels alone is estimated at 83,700,000 francs (£3,348,000) and 76,000,000 francs (£3,040,000) respectively.

THE number of railways to be worked by electricity is now considerable. Those which are working, authorized, or in course of construction, show a total length of about 100 miles. The lines actually at work are those of Lichterfelde, 1.56 miles, and that from the Spandauer Bock to Charlottenberg, near Berlin; the Port Rush to Bush Mills, in the North of Ireland, about 6.5 miles, and also in Holland, one from Zandvoort to Kostverloren about 1.3 miles long. Among lines authorized or in construction, the following are noted: In Austria, the Mœdling line, near Vienna, 1.5 miles, to be constructed by the Southern Railway Company there. In Germany, the line from Wiesbaden to Nürnberg, 1.3 miles, and that from the Royal mines of Saxony to Zankerode. The line under the Thames connecting Charing-cross and Waterloo stations will be about three-fourths of a mile; also a line in South Wales thirty-seven miles, for which the power will be derived from fall of water. In Italy, Turin and Milan will soon begin the construction of electric tramways. In America the Edison Company have arranged for the working of 80 kilometers on one of the great lines from New York. Another small line, 1.1 miles, is to be made at St. Louis, in Missouri, by Mr. Heisler.

CANADIAN RAILWAY PROGRESS.—The Canadian Pacific main line is constructed for a distance of 440 miles west of Winnipeg, of which about 300 miles have been built since May by Messrs. Langdon, Shepard & Co. The whole distance was built within fifteen months. The southwestern branch of the Canadian Pacific is now built two miles south of Morris, and the Selkirk branch is also under construction. Mr. Van Horne, general manager of the Canadian Pacific, states that the whole of the line north of Lake Superior will be under contract by January 1, 1883 and that next season work on the western division will be completed to the Rockies. Mr. Senecal is said to contemplate extending the North Shore Railway to Tadoussac, and establishing a new line of steamers from that point, thus making it the winter port of the western provinces of the Dominion. An era in the progress of Ottawa has been marked by the opening of the Canada Atlantic Railway extending from that city to Coteau Landing. The new line considerably shortens the route from Ottawa to Montreal.

NEW LOCOMOTIVES.—A locomotive on the compound principal has recently been perfected by Mr. E. W. Webb, Locomotive Superintendent of the London and Northwestern Railway. There are two sets of driving wheels, which are not connected, but each set is driven by independent engines. The steam is first received, under high pressure, in two cylinders, placed on each side of the boiler, and about half way back. The pistons of these work the rear drivers by direct motion. The steam from them is then exhausted into pipes, which are conducted in front of the boiler flues, to keep the steam hot, and then into one large cylinder located directly under the front of the boiler. This cylinder carries, by a shaft under the boiler, the forward drivers. A saving of over 25 per cent. upon ordinary requirements is said to be made in coal. Another fuel-saving locomotive is being tested on the St. Louis and San Francisco Railroad, which is known as the shut-down engine. In a trip from Springfield, Missouri, to the mouth of the tunnel through the Boston Mountains at Winslow, in Arkansas, and from there to Wichita, Kansas, returning to Springfield, a distance of 750 miles, it attained a speed of 50 miles an hour. To this engine is attached a smoke consumer and smoke arrester, and during the trip above referred to, no smoke was observable while the engine was working, though some could be seen to escape when the steam was shut off. It is stated that the consumption of fuel was at least one-third less. Still another novelty in America is Strong's express locomotive, which, says the *North American*, bids fair to spring into general use on every road where its merits are properly appreciated. The designer of the engine spent a long time in England and on the Continent, taking note of all the good points in locomotives used abroad, and, upon his return to the United States, constructed an engine embodying the results of his studies. In the boiler of Mr. Strong's locomotive the corner bars and side stays are done away with, the fire-box is designed to ensure complete combustion of fuel by burning the gases and sparks, the driver coupling is so arranged that the side rod is unnecessary, and there is also a better

distribution of wearing surface on brasses or crank pins than is the case in ordinarily constructed locomotives when the power for both wheels is transmitted through the forward pin. The valve motion is also improved, and the feedwater heated by a portion of the exhaust. In short, the locomotive is so constructed as to be economical, and as fast as is desirable, and always to have a reserve of power for a heavy train, while at the same time it is simple and not liable to derangement, and safe for those who run it as well as those who ride behind it, and one that shall burn its coal in so perfect a manner as to do away with cinders and smoke.

TUNNEL AND ELEVATED RAILWAY AT HAMBURG.—The Senate of Hamburg have had under their consideration a scheme, proposed by Herr Westendarp, an engineer, for constructing a tunnel under the Elbe, and an elevated railway in Hamburg. The construction of a bridge, instead of the tunnel s out of the question, owing to the width and crowded state of the harbor; and the tunnel must be laid deep enough not to interfere with the passage of deep draught vessels. The author of the project has designed the tunnel of such dimensions as would provide both for vehicular and foot passenger traffic, and a double-track railway for goods and passengers. This he proposes to effect by building the tunnel of two stories—the road for vehicles and pedestrians to be in the upper, the line of railway in the lower story. The crown of the tunnel for a length of 656 feet (the width of the navigable channel of the Elbe), is to be 20 feet below the new, or about 30 feet below the old low-water mark. Separate openings are provided for railway and roadway, the ascent to be very gradual, 1 in 35. The tunnel is to be an iron cylinder lined with brickwork, the upper story resting upon columns placed between the two lines of rails. The latter would be in continuation of the track of the elevated railway in the city, which is to be of a decorative ironwork, and similar to that of the New York Elevated Railroad. Three stations are provided—at the Exchange, the Fischmarkt, and the St. Annens-Platz, where elegant pavilions are to be erected in the level of the line for taking up and putting down passengers. The tunnel is to be lighted by electricity, and ventilated and drained by powerful machinery; provision is also made for taking through the tunnel gas and water pipes, telegraph and telephone wires, a pneumatic post, &c. The estimated cost of tunnel and railway is 26,000,000 marks (£1,300,000), and they are to be completed in 5½ years. As it is of the greatest importance to the city of Hamburg to connect the territory of the projected free port on the left bank of the Elbe direct with the city, that expense will not stand in the way of carrying out the stupendous undertaking.

ORDNANCE AND NAVAL.

VICE-ADMIRAL VON HENK—for some years at the head of the constructive branch of the German Navy—has published an article in a military periodical on the subject of iron-clad ships. He considers that experience and observation up to date prove: First, that unarmored ships cannot maintain a fight of any duration against the heavy guns of ships and forts. Secondly, that iron-plating is still an effectual defense against the heaviest guns, and consequently indispensable for battle-ships. Thirdly, rams and torpedoes are, indeed, formidable weapons in sea fighting, but cannot supersede artillery as the chief means of defense.

The above conclusions are arrived at from a review of the history of naval warfare since the introduction of shell guns. Admiral Von Henk considers it desirable to draw as many lessons as possible from every event in this field, and thus increase our experience; because military history has as yet furnished but few materials for forming a final judgment as to the value of armor-plating for ships, the purposes aimed at in its use, and whether these purposes have really been attained. He commences at the year 1850, soon after which shell guns began to be employed as naval weapons. Their use soon diminished the value of the old line-of-battle ships carrying 80 and 100 guns each, while the introduction of steam made the construction of ships more complicated and increased their vulnerability. This was shown conclusively in the Crimean War, when many of the best Turkish line-of-battle ships were set on fire and burnt by the Russian shells. Admiral Von Henk goes on to the American war, in which the Merrimac attacked four wooden frigates and destroyed two of them, but the next day was obliged to retreat, severely damaged, after an engagement with the ironclad ship Monitor. At the battle of Lissa, when seven Austrian ironclads were opposed to eleven Italian, the armor of the ships was about equal in strength on both sides, but the Italians were stronger in artillery. The relative strength of armor and guns was then about what it is now. The loss on board each of the armor-clad ships was very small, being only thirty-three killed on the Austrian side out of 7,000, but in the unarmored ship Kaiser the casualties were twenty two killed and 124 wounded, or two-thirds of the whole crew. The Admiral says that the battle of Lissa was an eloquent advocate for the incipient system of armor-plating ships of war. In the engagement between the two British unarmored ships and the Huascar in 1877, the latter had to be allowed to escape on account of the danger of approaching her. As to the fish torpedo, he considers that we have as yet no experience of that new, malignant, and deceitful weapon, for the published reports of the Russo-Turkish war are so untrustworthy that one can found no opinion upon them.

Coming to the bombardment of Alexandria, Admiral Von Henk says that several of the ships were hit and damaged. The flagship Alexandra suffered most. She had fourteen shots in her hull. One shell went through her deck and burst in the Admiral's cabin, another in the captain's, while others went through the funnel and shattered the long boat. The Sultan and Superb were also severely punished by the enemy's shot. The Superb got twenty-three shots from Fort Ada, though most of the

shots from this fort fell either short of, or beyond the ships. The reason why the Condor and other gunboats suffered so little is partly on account of their small size, but chiefly because of the unskilfulness of the Arab gunners. The position of the ships was unfavorable, as the sun interfered with the aim of the gunners, and the wind was also against them. After the first volley a cloud of smoke arose and made it difficult to see from the ships whether their shots took effect. Only from the top of the masts could it be judged approximately whether and how much, each shot fell short of, or went beyond, its mark. The same difficulty arose after each volley. The 1,700 lb. shells of the Inflexible are said to have had a most demoralizing effect upon the Egyptian soldiers, but special notice is due to the great resistance offered by the earthworks to the fire from the ships. The loss of the Egyptians is unknown. Although so many shots went through the ships, the only penetration was in the unarmored non-fighting portions. Not one pierced the armor of any ship. The total English loss was five dead and twenty eight wounded. Neither attack nor defence were, however, normal, for it admits of no doubt that if the English ironclads had been opposed to European forts, with modern guns, skilled gunners, and a number of swift torpedo boats and of sea mines, they would not only have had a hard time of it, but would have suffered much damage and loss of life.

Admiral Von Henk concludes that the lessons to be drawn from the bombardment of Alexandria are the following: 1. The great value of armor-plating for protecting the men, the engines, the gun platforms, and the most vital inner parts of the ships. 2. The necessity of using the heaviest guns first, at the beginning of the attack, in order to diminish the difficulties of aiming well, manœuvering, and guarding against torpedo boats, caused by the smoke.

These opinions agree with those expressed by us, as already referred to, and will doubtless form subjects of discussion for some time to come among naval men. The smoke difficulty is a serious one, and has been but little considered. It is certain that the dense smoke produced as soon as firing commences will embarrass very much the evolutions of the various ships of a fleet, and often make it very difficult for them to manœuvre and attack with precision. This will give the opportunity for fast torpedo boats and rams to rush in unperceived and give the *coup de grace* to large and heavy ironclads. The best position for a look-out in these circumstances has yet to be discovered; and also the best mode of making it somewhat secure. The tops furnished the most obvious and available means for the purpose at Alexandria; but their frailness and insecurity disqualify them from being anything more than a perilous and temporary refuge. The matter is deserving of the careful attention of naval men, and the conduct of future naval actions will depend largely upon it.

The high value of artillery as the primary offensive weapon in ships admits of no question, particularly after such an experience as we have had at Alexandria, where the gun was the only arm which could be employed. The ram and torpedo are effective and deadly weapons, but must be subordinate to the gun, and in this case the former would have been useless, however well our ships may have been supplied with them. It is suggestive to note that Admiral Von Henk calls the fish torpedo a deceitful weapon. The Germans have had great experience in its manipulation, and have experimented with it on a large scale; but it would appear that they have not any great confidence in its practical value.

The remarks upon the great value of armor-plating in protecting the lives of the crew, and the machinery and fighting appliances of a ship, form an interesting commentary upon Sir W. Armstrong's presidential address to the Institution of Civil Engineers in January last, when he advocated that ships of war should be constructed without armor-plating, and made "freely penetrable." He argued that as it is possible to make a gun that will pierce a target composed of the thickest armor a ship can carry, armour-plating is therefore devoid of protective value; and that as unarmored ships can be so divided into compartments as to be practically unsinkable, armor-plating is not necessary to preserve them from destruction. These arguments, as we showed in our issue of 20th of January last, only show how narrow and erroneous the views of specialists often are, even though they may have risen to the eminence of Sir W. Armstrong. We then pointed out, and the recent action has proved it, that armor has a very high defensive value, even though it may not make a ship absolutely invulnerable against any imaginable attack—that it is a great protection to life, and against the disablement of the guns. It is of no use to make a ship unsinkable if she can be penetrated through and through by the enemy's shells, and the crew and fighting appliances destroyed. The bombardment of Alexandria will lead to a more correct appreciation of the value of armor than has formerly been general; as it is certain that but for what Sir W. Armstrong would call the "obsolete" armor-plating of our ships, there would have been very many more casualties and much damage done.

The German admiral takes an impartial, correct and practical view of the question; and is right in stating—what is after all an unfortunate necessity—that armor-plating is still an effectual defense against the heaviest guns, and consequently indispensable for battle ships.

IRON AND STEEL NOTES.

PROGRESS OF THE BASIC PROCESS.—At a meeting of the South Staffordshire Mill and Forge Manager's Association, held at Dudley on November 25, Mr. R. Edwards in the chair, a paper containing the latest information on the basic steel process, by Messrs. Thomas and Gilchrist, was read by Mr. P. C. Gilchrist. The authors remarked that the common Staffordshire pig-iron with which they experimented last June at Wednesbury had, before being melted, the following composition:—Strong forge pig—manganese, 1.12 per cent.; silicon,

1.17; sulphur, 0.08; phosphorus, 2.07; grey forge pig—manganese, 1.13; silicon, 1.67; sulphur, 0.5; phosphorus, 2.72. The strong and grey forge were mixed in equal proportions and melted in the air furnace, and the resulting metal contained—manganese, 0.75; silicon, 1.28; sulphur, 0.10; phosphorus, 2.94. The authors presented the analysis which they had specially obtained from Mr. Windsor Richards, and which had been made by Mr. E. W. Cook, the chemist to Bolckow, Vaughan & Co., of the pigs which that company used up in the basic process on the Thursday previous to the meeting November 23, and analyses of the rail made from it, and of the basic brick used for the lining of the converters on that day. The analyses of the brick was:—Lime, 49 91 per cent.; magnesia, 30.72; alumini, 4.50; oxide of iron, 3.46; and silica, 11.41. The analysis of the pig used was:—Iron, 92.85; combined carbon, 1.10; graphite, 2.25; manganese, 0.60; silicon, 1.30; sulphur, 0 15; phosphorus, 1.75. The rail analysis was:—Iron, 98.25; combined carbon, 0.46; manganese, 1.18; silicon trace, sulphur, 0.05; phosphorus, 0.060. The casket of basic steel which the authors exhibited as having been presented to them by the Central Director of the Kladno Steel Works, Austria, was made from pig containing carbon, 3.03; silicon, 1.06; phosphorus, 1.86; manganese, .48; and sulphur, 0.26. The ingot iron, of which the casket was wrought, contained carbon, 0.18; silicon, traces; phosphorus, 0.05; manganese, 0 34. The authors further stated that Messrs. Bolckow, Vaughan & Co. were now making 9,600 tons of basic steel per month, and that in January next they expected to make 16,000 tons. At present they had four 12-ton converters. But the Continental steel masters were still ahead of England in their appreciation of the process. The authors had obtained returns from the Continental Works, showing their individual output under this process as recently as during the last month (October). These showed a total production of 37,639 tons of steel. This was produced at one work in France, one in Belgium, eight in Germany, three in Austria, and one in Russia. The largest individual output during October at any one of these works was 7,000 tons, being at the establishment of the Dortmund Union, who were employing two converters of 9½ tons each. The next largest output was at the Hoerde Works, Germany, which possesses three 10-ton converters. Their output was 4,100 tons. The process was likewise extending more rapidly on the Continent than in England. In Europe 25 converters were now being built, with a minimum capacity of 36,000 tons per month; while in England nine converters were being erected, which would probably produce at least a further 16,000 tons per month.—*Iron.*

EXPANSION OF STEEL IN HARDENING.—It is well known to workers of steel that by the process of hardening it is sometimes expanded, so that in a job where two pieces are fitted accurately in their unhardened state they become so much enlarged that grinding is necessary to restore them to their former relative size. No rule has yet been found by which the amount of this expansion can be actually determined before hardening. It may depend upon the amount of carbon in the steel, which varies in different specimens; on the degree of heat employed in the process; and also largely on the size and shape of the pieces treated. This expansion, may, however, be reduced to the minimum by repeated annealings while the work is in progress at the lathe, vise, or planer. It is also true, that in some instances steel, having been hardened, will contract slightly when drawn to temper, the amount of contraction depending on the lowness of drawing.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

ABSTRACTS OF PAPERS IN FOREIGN TRANSACTIONS AND PERIODICALS FOR THE INSTITUTION OF CIVIL ENGINEERS.

PROCEEDINGS OF THE SOCIETY OF ARTS, MASSACHUSETTS INSTITUTE OF TECHNOLOGY, FOR 1879–80; 1881–82.

JOURNAL OF THE ASSOCIATION OF ENGINEERING SOCIETIES.

REPORT OF THIRD MEETING OF MICHIGAN ASSOCIATION OF SURVEYORS AND CIVIL ENGINEERS.

ANNUAL REPORT OF CHIEF ENGINEER OF THE WATER DEPARTMENT OF THE CITY OF PHILADELPHIA.

ANNUAL REPORT OF THE CHIEF OF ENGINEERS OF THE UNITED STATES ARMY FOR 1882.

ENGINEERS' BOOK OF TABLES FOR RAILWAY CONSTRUCTION. Compiled for Engineering Department of New York, West Shore and Buffalo Railway, and the New York, Ontario and Western Railway.

DIE ELEKTRISCHE BELENCHTUNG. Von A. Merling. Braunschweig: Vieweg & Sohn.

This is a 12mo book of 500 pages, abundantly illustrated and describing the leading types of Magneto and Dynamo machines, and the various systems of arc and incandescent lighting.

There is little if anything new in the volume, but to readers of the German language it offers a large amount of information in compact form.

ELECTRO METALLURGY. By Alexander Watt. New York: D Van Nostrand.

To state that the present is the sixth edition is to say that the book has proved a valuable aid to those engaged in galvanoplastic operations.

Its chief value lies in the fact that all directions for working are remarkably specific. There is no better guide for amateurs.

The present edition presents considerable new matter, chiefly relating to the deposition of nickel.

BESSEMER STEEL. Ores and methods. Thomas W. Fitch. St. Louis: Morrison Renshaw.

The author prepared the papers here published at the request of the Miners' and Manufacturers' Association. They are statistical in character and in brief, compact form, suitable for newspaper articles in accord with the original intention of the author.

The Ores of the United States, Great Britain, Germany, France, Belgium and Sweden form the subject of the first chapter. Chapter 2d describes the American Bessemer processes, and chapters 3d and 4th are devoted to the methods in foreign countries.

The Basic Bessemer process forms the subject of of the 5th chapter, and the Harrison Steel Company of the 6th and last chapter.

No illustrations are given, and probably none are needed by the technical readers who alone can read it with profit.

THE THEORY OF EQUATIONS. By William Snow Burnside, M. A., and Arthur William Panton, M. A. London: Longmans, Green & Co.

This book will be welcomed by students of Algebra who desire to supplement their college course of Mathematics. It is unlike modern treatises on the newer branches of research, inasmuch as it demands no familiarity with newly coined terms, and because the methods are familiar ones of old-fashioned Algebra.

The typography is exceedingly good.

THE RAILROAD SPIRAL. By Wm. H. Searles, C. E. New York: John Wiley & Sons.

This neat little book is for the use of Railway Engineers only. It affords by its formulas and tables a ready way of easing circular curves at their tangent points.

The method involves the use of the transit and chain as in the ordinary way of location, and is a scientific substitute for "the rule of thumb" methods largely employed at present.

THE BUILDER'S GUIDE AND ESTIMATOR'S PRICE-BOOK. By Fred T. Hodgson. New York: Industrial Publication Co.

Estimates for all materials used in building are presented in tabular form, together with simple rules for calculating the dimensions of such parts as are subjected to strains, make up the most of this convenient little hand-book.

Some tables of weights of materials and for timber measure are properly added for convenience of the artisan.

NAVAL HYGIENE. By John D. Macdonald, F. R. S. London: Smith, Elder & Co.

That ships are capable of hygienic improvement any one must be convinced who has ever been at sea. That the problems to be solved involve conditions not met with in dwellings is also apparent upon slight reflection.

The writer has certainly presented the subject in a way that indicates a laborious study of it, and seems to have treated it exhaustively. The topics are in order as follows: Conservative Hygiene treating of Structure of Ships; Ventilation of Ships; Water Supply; Cleanliness; Diet; Exercise and Clothing.

Trophylactic Hygiene and Remedial or Corrective Hygiene. The last two sections are more especially for ship physicians

The diagrams are numerous and instructive.

HOUSE DRAINAGE AND SANITARY PLUMBING. By Wm. Paul Gerhard. Science Series No. 63. New York: D. Van Nostrand. Price, 50 cents.

This is a carefully written essay designed for the household quite as much as for Sanitary Engineers.

The defects of the ordinary method and the most modern improvements are presented with satisfactory fullness.

Dwellers in houses everywhere can gain profitable knowledge from this little treatise.

THE THEORY OF THE GAS ENGINE. By Dugald Clerk. Science Series No. 62. New York: D. Van Nostrand. Price, 50 cents.

The interest in the Gas Engine in no way diminishes, as the prospect of cheap gas fuel seems more promising.

The economy of space as well as of fuel has long since been demonstrated, and in this little essay the theory upon which its future success is predicated is plainly set forth.

MISCELLANEOUS.

TRIPOLITH.—Tripolith is the name given by its inventors to a new binding material for builders, a substitute for lime, cement and plaster under certain circumstances, and which is composed of sulphate of lime, coke, and oxide of iron in some form or other. That the material requires considerably less water to form a workable mortar than ordinary lime is no doubt an advantage, while the time for setting can be admirably regulated by adding more or less ordinary slaked lime. Thus while tripolith mixed with sand only, sets in 10 to 15 minutes, an addition of slaked lime may easily increase the time required for setting to 60 minutes. The specific gravity of tripolith is lower than that of plaster, the former is 1.678, the latter 1.696. Turning now to the tests we give in our Table the mean results in each case of five complete experiments. The extraordinary increase of tensile strength after a long exposure to the atmospheric air is remarkable; it amounts to 100 per cent from seven to ninety days in mortar B, and to 189 per cent in mortar C for the same time. Compared with the tensile strength of lime and cement, the results obtained with tripolith are highly satisfactory. The compression tests point out for tripolith a position between lime mortar and cement mortar, but since after being fairly set it acquires about the same crushing strength as ordinary bricks, no more would be needed for general use. In sifting, tripolith mortar loses in weight, and when placed in water does not absorb the latter so rapidly as ordinary mortar does. Its adhesion to brick, stone, and other materials is very considerable, and the tripolith mortar does not either reduce or increase noticeably in volume when setting. For facing and plastering, this material is excellently suited; it is easily handled and smoothed while soft, adheres well to brick or

stone surfaces, and attains far greater hardness than plaster-of-paris, and oil or other colors adhere to it well.—*Engineering.*

THE NEW EXPLOSIVE.—At the bi-monthly meeting of the North Staffordshire Mining Institute, Mr. Storey, of Kidsgrove, read a paper on Smith and Morris' process of getting coal by caustic lime. He had seen the new system in operation at Shipley, and he minutely described it. The new agent consisted of lime in cartridges in a special caustic state, and the experiments were made in the hard coal, where there was a good roof, and the seam was almost flat, the work being done on the long wall system. The circumstances under which lime was used at Shipley differed so much from those of North Staffordshire mining that he feared that it could not be successfully applied to many of the mines of the latter district, where the roofs were not so good as at Shipley. Neither had they the chance of undercutting to be found where there were hundreds of yards of long-walled face. There were other disadvantages in North Staffordshire, as compared with Shipley, but any method which would enable them to dispense with blasting would be heartily welcomed, and he should like to see the lime process fairly tried in the district. In the discussion which followed, a general wish was expressed that the new explosive should be fairly tested. A discussion also took place relative to the properties of fire-damp and coal dust, and several recent instances of coal dust firing were mentioned. Mr. Lawton said they were without the means of detecting such a small quantity of fire-damp as Professor Abel said was dangerous when mixed with coal dust. He urged that further experiments should be made to obtain the requisite knowledge with which to guard against the mischievous effects of fire-damp. Mr. Young said he used salt as a means of laying the dust in the most dusty seam in the district, and it answered well. Mr. Macdonald said, if salt was used for laying the dust near where the shots were fired, if any explosion occurred, its effects would be localized. Several other members joined in the debate.

EGYPTIAN AND CHINESE LOCKS.—The earliest lock of which the construction is known is the Egyptian, which was used 4,000 years ago. In this lock three pins drop into three holes in the bolt when it is pushed in, and so hold it fast: and they are raised again by putting in the key through the large hole in the bolt and raising it a little, so that the pins of the key push the locking pins up out of the way of the bolt. The security of this lock is very small, as it is easy to find the places of the pins by pushing in a bit of wood covered with clay or tallow; on which the holes will mark themselves; and the depth can easily be got by trial. Mr. Chubb, the English lock-maker, possessed a wooden Chinese lock, which is very superior to the Egyptian, and in fact, is founded on exactly the same principle as the Brahma lock, which long enjoyed the reputation of being the most secure one ever invented; for it had sliders or tumblers of different lengths, and could not be opened unless they were all raised to the proper heights, and no higher.

NICKEL VERSUS BRONZE.—A financial committee of inquiry, appointed to consider the question of substituting a nickel for the bronze coinage at present in use in France, has finally decided in favor of the project, which, it may be mentioned, has already been adopted by other countries, and notably by Germany, Belgium, and Switzerland. The work thus thrown upon the mints of Paris and Bordeaux will be gigantic, it being estimated that there are 500,000,000f. worth of bronze coins in circulation; but the necessary appliances are already in hand, and the work will be rapidly proceeded with. It should be known that coins of copper-nickel have been introduced in North America, Peru, Brazil, and Honduras; and in 1869 Professor Graham, the last Master of the Mint, issued a coinage of the same kind for the Island of Jamaica in penny and halfpenny pieces, and very handsome coins they are. It has been pointed out by Dr. Walter Flight, of the British Museum, South Kensington, in the *Journal* of the Chemical Society for April of this year, that he was informed by Professor Graham in 1869 that he should have advocated the issuing of a coinage of the same kind in the British Isles if only a sufficient supply of nickel could at all times be obtained. Our sources of nickel have been materially increased during the twelve years interval. It is a matter of no little interest, as pointed out by Dr. Flight, that more than 2100 years ago copper-nickel coins were used in Bactria by the kings Agathokles, Pantaleon, and Euthydemus, the composition of which were identical with those now coined, although nickel was only discovered in 1751 by Crondstadt.—*The Engineer.*

IN their sixth annual report, Colonel Majendie, and Major A. Ford, the Inspectors of Explosives, say: "Experiments conducted by us appear to establish very satisfactorily that the effect of small charges of dynamite, and similar explosives, upon masonry structures is essentially local. Where the charge is in contact with an external portion of the structure, any effect which may be produced is almost entirely confined to a complete or partial penetration of the structure at the spot where such contact occurs; while if the charge be not in contact with any part of the structure, the result in the case of an external explosion is either wholly or nearly negative, while if occurring in the interior of a building any effect which may be produced is limited to the more or less complete demolition of the chamber or portion of the structure in, or in the immediate neighborhood of which the explosion was effected. General or even partial destruction of a public building, or of a substantial dwelling house could not be accomplished except by the use of very much larger charges of dynamite and similar substances than could usually be brought to bear without attracting observation, and the effect of a single 'Infernal Machine,' containing a few pounds of explosive would be structurally insignificant."

FURNACE SLAG AND BAUXITE FOR CEMENT.—We learn from *Stahl and Eisen* that Herr Roth, mining engineer, of Wetzlar, uses bauxite in the manufacture of cement from blast

furnace cinder. Bauxite consists principally of alumina hydrates, besides small quantities of sesquioxide of manganese, titanic acid, lime, magnesia, alkali, &c.; but its chemical composition varies according to the localities where it is deposited. Its name is derived from the place where it was first discovered, Les Baux, in France: it also occurs in the Charente. In Italy it is found in Calabria; in Ireland, near Belfast; in the Austrian Empire, in Krain, Styria, and Lower Austria. In Germany bauxite occurs on the southern slope of the Westerwald near Mühlbach and Hadamar, also at the Vogelsberg, in Upper Hessen, and at Klein-Steinheim, near Hanau. If 100 parts of furnace cinder, which crumbles by itself, are mixed with 85 parts of limestone or chalk (containing 98 per cent. of carbonate of lime and 2 per cent. of silicic acid) and 15 parts of bauxite (containing 48.5 per cent. of alumina, 13.52 per cent. of sesquioxide of iron, and 9.40 per cent. of silicic acid; the composition of the bauxite found near Giessen), and burned, the product yielded—supposing that half of the sulphur escapes from the slag as sulphureted hydrogen—is 158.66 parts of cement of the following composition: Lime, 61.9 per cent.; silicic acid, 24.1 per cent.; alumina, 10.6 per cent.; sesquioxide of iron, 1.3 per cent.; protoxides of iron and manganese, 0.8 per cent.; magnesia, 1 per cent.; sulphur, 0.3 per cent, The cinder used was obtained in the production of foundry pig in a coke blast-furnace; if the cinder to be employed is of a different composition, the fluxing materials must be varied. Herr Roth demonstrates the economical advantages to be derived from the erection of special cement mills near blast furnaces.

MR. A. J. HADCOCK, A. Inst. Chem., recently related the following: A kettle filled with boiling water was hung in the hottest room of some Turkish baths with the lid on. The temperature of the surrounding air was 262 deg. Fah. After about an hour the temperature of the water was taken, and indicated, as was expected, 212 deg. The kettle was then re-hung with the lid off. The temperature of the room was now 252 deg. In twenty minutes the temperature of the water had fallen to 185 deg., in thirty minutes to 178 deg., in forty-five minutes to 170 deg., and was evidently still falling. The manager stated that it generally fell finally to about 140 deg., when a point of equilibrium seemed to be established, and the water neither got hotter nor cooler. Mr. Hadcock supposes the loss of heat was due to rapid vaporization, and conversion of the sensible heat of the water into the latent heat of steam, and as dry air is a very bad conductor of heat—one of the worst known—the heat required to convert a portion of the water into steam had to be abstracted from the remainder of the water, thus lowering its temperature. In substantiation of this explanation it is well known that if water is placed in a vessel over a large bulk of strong sulphuric acid, in the receiver of an air-pump, and the air is exhausted, the rapid evaporation of one portion of the water will actually cause the rest to freeze.

THE *Nautical Gazette* says that during the year 1881 the vessels lost at sea averaged about one every four hours. A large proportion of these losses occurred from carelessness, and mostly in fogs and other darkness. There were 400 ocean steamer collisions in 1879 and 1880 in the North Atlantic Ocean alone. Each of these might have been avoided if the master of one colliding vessel had been informed in proper time of the course pursued by the approaching one. These losses gave an average of over one steamer a day in which human life was sacrificed and valuable property destroyed. The *Gazette* believes that if a system of fog signals had been in use, such as the Barker code, nearly all of these disasters would have been prevented or avoided.

FOR cleaning old and soiled engravings, Mr. W. Brooks, writing in the *Journal of Photography*, recommends the use of Holme's ozone bleach. The strength he prefers is one part of ozone bleach to ten of water, well shaken up before pouring into a dish. He immerses the engraving in the solution, face upward, avoiding bubbles. The only caution to be observed is that when the engraving is sodden with water it is somewhat rotten; so the less it is handled the better. Sometimes, if the engraving be only slightly stained, half an hour is quite sufficient, but when quite brown he has left them in for as long as four hours. After all the stains are removed, and the paper has regained its pure whiteness, pour the solution out of the dish into a bottle, as this can be used over and over again, until it becomes discolored; then fill up the dish with water, changing frequently for about two hours, or, better still, place it in running water. When sufficiently washed it can be taken out and blotted off and then hung up to dry, and when perfectly dry, iron on the back with a warm flat-iron; but care must be taken not to have it too hot.

THE annual iron produce of the world is calculated from the most recent statistics to yield some 19½ millions of tons. The yield from all the more important countries has been ascertained up to the year 1881. In regard to the others, it is assumed that the yield has not fallen off since the latest figures reported. For the year 1881 the yield of Great Britain was 8,377,364 gross tons; United States, 4,144,254; Germany, 2,863,400; France, 1,866,438; Belgium, 622,288; Austria-Hungary, for 1880, 448,685; Sweden, for the same year, 399,628; Luxembourg, for 1881, 289,212; Russia, 231,341; Italy, for 1876, 76,000; Spain, 73,000; Turkey, 40,000; Japan, 10,000: and all other countries, 46,000. Under "other countries" are included Canada, Switzerland, and Mexico, each producing about 7,500 tons per year, and Norway, with 4,000 tons per year. The grand total is 19,487,610. Great Britain, the United States, Germany, and France, produce no less than 88.4 per cent. of the world's iron supply; the first two 64.3 per cent., and Great Britain alone 43 per cent. The chief consumer is the United States, taking 29 per cent.; Great Britain comes next with 23.4 per cent.; and these two use more than half the whole supply.